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THE QUARTERLY JOURNAL OF EXPERIMENTAL PSYCHOLOGY

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THE QUARTERLY JOURNAL OF
EXPERIMENTAL PSYCHOLOGY

Vol. III 1951 Part 2

SENSORY PHENOMENA IN EXPERIMENTAL NERVE BLOCK

BY

D. C. SINCLAIR AND J. R. HINSHAW*

(*From the Department of Human Anatomy, Oxford University*)

In the course of a series of blocks of the ulnar and lateral popliteal nerves of volunteers a study was made of the manner in which the responses of the subject to sensory stimulation become progressively altered and finally obliterated. These changes are described, together with the manner of recovery, and an account is given of various sensations induced by the blocking and the recovery processes. The relevant literature is briefly reviewed and possible factors involved in these phenomena are discussed.

I

Introduction

It has long been known that the normal sensibility of an area of skin can be progressively reduced and eventually abolished by the application of various physical or chemical agents to the nerve trunk supplying that area. This knowledge remains largely empirical, and there is little information as to the exact mode of action of these anæsthetic agents. Nevertheless, the production of regional or local anæsthesia has been used as an experimental tool in the study of the mechanisms of cutaneous sensation.

Under these circumstances, the different types of sensory experience tend to disappear serially rather than simultaneously, and the study of sensory dissociation of this kind contributed materially to the acceptance of four primary modalities of sensation by von Frey and his school. Indeed, the main object of the earlier workers was to demonstrate that the disappearance of a given "modality" of sensation was a process independent of the disappearance of the others. Subsequent investigators using the method have tended to concentrate their attention on particular details, such as the relation of the disappearance of two-point discrimination to the time of disappearance of touch sensibility, or the relationship of itching to pain sensibility. The result has been that no full account has appeared of many phenomena "incidental" to such experiments. These phenomena often appear to be inherent in the process of nerve block, however it may be initiated, and a study of them may well throw light on a number of problems connected with cutaneous sensation. It is the intention of this paper not to deal exhaustively with any one of them, but rather to provide as complete a picture as is possible at present of the various sensory findings in a series of nerve blocks induced by procaine, cold and compression of the limb.

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II

Material and Methods

The 31 subjects used were either medical students or medical research workers; only four had any previous experience in the assessment of cutaneous sensation. The skin areas investigated were those supplied by the ulnar nerve and the lateral popliteal nerve.

Technique of inducing blocks.

For the procaine blocks a solution of procaine hydrochloride in strengths varying from $1\frac{1}{2}$ to $2\frac{1}{2}$ per cent. was injected round, but not into, the ulnar nerve as it passes behind the

TABLE I

Number of Blocks

Subject	*Ulnar blocks*			*Lateral popliteal blocks*		
	Procaine	*Cold*	*Compression*	*Procaine*	*Cold*	*Compression*
C. A. W. ..	1					
J. H. F. ..	1					
H. B. ..	1					
I. A. Y. ..	1					
R. H. P. ..	1					
H. M. H. ..	1					
J. P. R. ..	1					
M. E. ..	1					
D. H. C. ..	1					
K. M. O. ..	1					
R. B. E. ..	1					
W. F. B. ..	2	1				
L. W. ..	2	1				
C. W. ..	2	1	1	1		1
R. I... ..	2	1	1			
D. S. ..	1	1	93		1	4
W. C. ..	1	1	1			
J. D. W. ..	1	1	1			
R. W. B. ..	1	1	1			
D. R. W. ..	1	1	1			
J. R. H. ..		1	1		1	
D. G. C. ..			1			
R. J. ..			1			
C. B. ..			1			
W. H. F. ..			17			
G. W. ..			21			
A. W. D. ..			15			
R. B. I. ..				1		
E. J. B. ..				1		1
G. A. R. ..				1		1
R. B. E. ..				1		1
Total ..	24	10	156	5	2	8

elbow, or the lateral popliteal nerve as it passes over the neck of the fibula (Sinclair & Hinshaw, 1950).

The compression blocks were induced by inflating a sphygmomanometer cuff 12 cms. wide round the arm just above the medial epicondyle of the humerus, or just below the knee-joint (Sinclair & Hinshaw, 1951a). The pressure used was 200 mm. Hg. in both cases; for the arm compressions the limb hung vertically at the side (Sinclair, 1948), while for the leg compressions the subject lay on his back with the leg horizontal.

Cold blocks of the ulnar nerve were induced by immersion of the elbow in a trough containing chopped ice and brine at a temperature of from 0° C. to minus 2° C. For the cold blocks of the lateral popliteal nerve, brine from a reservoir containing chopped ice ran slowly through a copper tube strapped over the neck of the fibula, the temperature in the tube being from 2° C. to 4° C. (Sinclair & Hinshaw, 1951b).

Table 1 shows the number of times each subject submitted to a given type of block. Most of the compression blocks were not maintained for longer than 20 minutes, and only 14 of the total of 164 were carried sufficiently far to enable a determination of the order of loss of sensory modalities to be made. On the other hand, the 12 cold blocks and the 29 procaine blocks were all taken to this point.

Technique of sensory examination.

Within the autonomous zone of the ulnar nerve, six circles 1 cm. in diameter were stamped out with marking-ink (Figure 1); for the most part sensory examination was confined to the three marked 1, 4 and 6 in the diagram. A similar set of three circles was

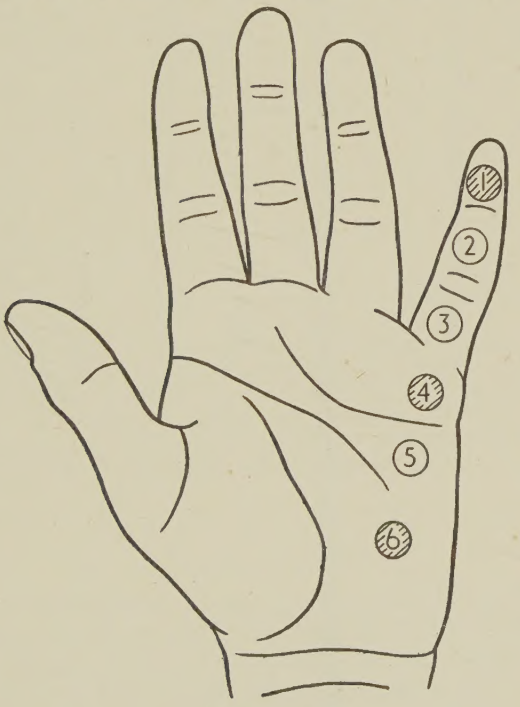

FIGURE 1

Sensory testing areas in the distribution of the ulnar nerve. Each area is 1 cm. in diameter and is stamped out on the skin with marking ink.

used for the lateral popliteal nerve (Sinclair & Hinshaw, 1950), and in this case the hairs in the region of the testing areas were clipped short (not shaved). This was done in view of the observed difference in behaviour between skin touch and hair touch in compression blocks (Sinclair, 1948). To these circles stimuli were applied at intervals, the order in which the circles were tested by any given stimulus being randomized. The reactions to four different stimuli were investigated. The first of these was a piece of nylon suture

material (Figure 2) delivering a punctate stimulus within the testing circle, care being taken to avoid skidding or slipping of the end of the nylon thread on the skin. Such a stimulus could invariably be appreciated in the ulnar testing areas, but the percentage response in the lateral popliteal areas varied considerably from subject to subject, and, if

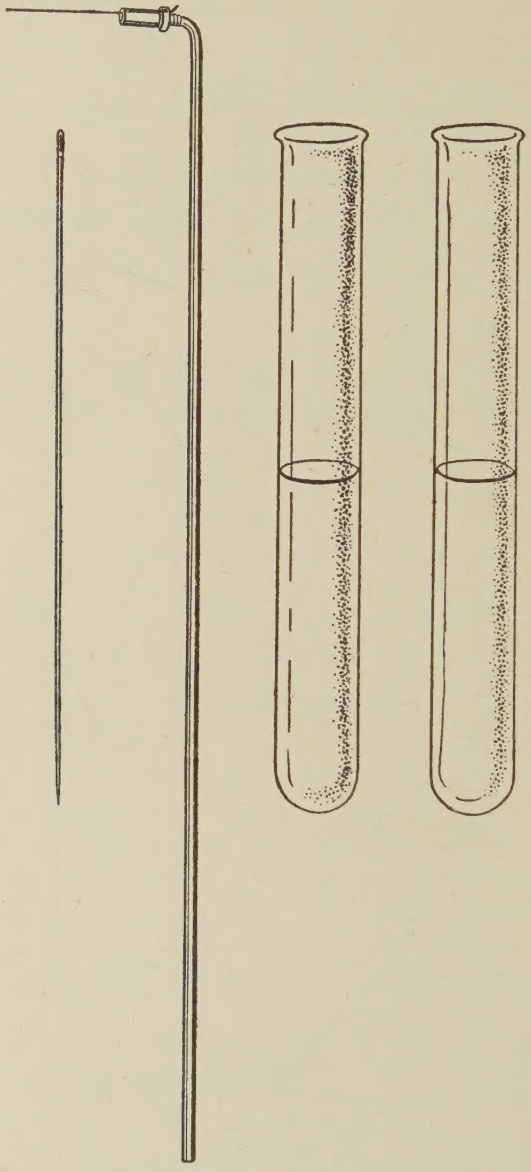

FIGURE 2

Standard Sensory Stimuli: The round-bodied needle is 12·5 cms. long. The piece of No. 2 nylon suture material is mounted on a holder 25 cms. long made from a bicycle-spoke; the free portion of the suture is 2·5 cms. in length, 0·17 mm. in diameter, and is capable of holding down a weight of 500 mg. The test-tubes are 1·5 cms. in external diameter and 12·5 cms. long, and are half-filled with water at 45° C. ± 2° C. and 15°C. ± 2° C. respectively. The surface of the tubes in contact with the skin is about 50 sq. mm.

the hairs were avoided, there was frequently no response at all; in such cases a heavier nylon thread was substituted. Alterations in the response to this stimulus were taken to reflect events taking place within the modality of touch.

Events within the modality of pain were investigated by the reaction of the subject to a vertical prick, just not drawing blood, from a round needle (Figure 2). The pricks were delivered at random within the boundary of each circle. This form of stimulus necessarily affects modalities other than that of pain; nevertheless it was taken as the "pain stimulus." In the normal subject the pricks were consistently stated to produce pain when applied to the ulnar territory: in the lateral popliteal area occasional pricks were identified merely as "sharp" or "prick without pain," and it might be necessary to draw blood before the subject would admit to a sensation of "pain."

The testing of temperature sensibility likewise involved modalities other than that of temperature. A glass test-tube half full of warm or cold water and containing a thermometer was allowed to rest by its own weight on the circle to be tested. The temperatures of the water were 15° C. ± 2° C. ("cold stimulus") and 45° C. ± 2° C. ("warm stimulus"). No measurement was taken of the temperature of the outside of the tube, but neither stimulus ever caused any reaction interpreted by the intact subject as pain. Normally, these two stimuli were interpreted as "cold" and "warm" over the greater part of the ulnar territory. Over the palmar surface of the distal phalanx (area 1), however, both were occasionally called "indifferent," and in some subjects it was difficult or impossible to distinguish between the two tubes in this area. In certain instances the findings were similar in the testing areas of the lateral popliteal territory.

Every subject was told that the object of the experiment was to investigate the way in which sensation disappears from an area of skin when its nerve supply is blocked, and warned that if he experienced any "spontaneous" sensations in the area blocked during the experiment he was to report and describe them as soon as they arose. He was told no details of what he might expect. During the testing periods he was told which modality was being tested—i.e. "touch," "pain" or "temperature," but was not permitted to watch the application of the stimuli. Routine precautions were taken to guard against the occurrence of "imaginary" responses (e.g. by pretending to test one modality and actually testing another, by announcing but not fulfilling a test, etc.). In the case of the temperature tubes in particular, every effort was made to conceal from the subject which tube was being used. The subject in every instance initially reported his recognition of the fact that a stimulus had been applied, and secondly described his experience in his own words, an abbreviated version of which was taken down in shorthand wherever possible. In the early stages these descriptions were often extremely clear and detailed, but at the height of the block they usually became reduced to the pathetic phrase, "I think there might be something going on somewhere"—a description which indeed it is impossible to better.

In addition to reporting the results of the standard stimulations, the subjects were often asked to describe the result of much grosser stimuli, such as stroking the whole hand, immersing it in hot or cold water, etc. They were also asked to compare the intensity and quality of the sensation caused by a stimulus applied to the blocked area with the intensity and quality of the sensation caused by the same stimulus applied to the mirror point on the opposite limb.

III

Observations

The general course of events in a typical compression block of the arm is set out in Table II, which is derived from a study of the notes on each individual experiment. The time intervals in this table are only approximate, but are given as a rough guide to the usual sequence. In Table III the phenomena noted are classified in terms of their relative frequency; not all of them appeared in every block, but many recurred with such regularity as to make them characteristic of the blocking process. Table III also shows the relative frequency of these changes in the procaine and cold blocks, and it will be noticed that every sensory manifestation occurring in either the procaine or the cold experiments had its counterpart in the compression blocks. On the other hand, the reference of a sensation to a distant point and the occurrence of positional hallucinations, which both occasionally took place in compression blocks, were not noted in either of the other two types of block.

TABLE II

SEQUENCE OF EVENTS IN AVERAGE COMPRESSION BLOCK

(Cuff on Arm above Elbow: Testing in Distribution of Ulnar Nerve)

Time		*Phenomena encountered*
0–5 mins.	(1)	Discomfort under cuff.
	(2)	Tingling referred to hand begins, usually after 1–2 minutes.
	(3)	Sensory testing difficult during tingling, but stimuli may be felt with greater intensity than in control hand.
5–10 mins.	(1)	Discomfort under cuff may pass off.
	(2)	Tingling subsides.
	(3)	Occasional spontaneous sensations of warmth and cold in the hand; occasional pain in the wrist.
	(4)	Little change in subjective response to stimuli.
10–15 mins.	(1)	Spontaneous brief sensations of temperature or pain may recur.
	(2)	Little apparent change in responses to relatively punctate stimuli. Gross stimuli (stroking with hand, etc.) disclose alteration in quality of touch. Hand "feels different," "velvety numbness" begins.
	(3)	Stimuli occasionally "referred" to distant area on same limb.
	(4)	Hand may feel swollen.
15–20 mins.	(1)	Position sense becomes impaired.
	(2)	Subjective responses to touch and pain stimuli begin to alter in quality and intensity.
	(3)	Paradoxical temperature sensations appear.
	(4)	Gradually increasing delay in perception of all stimuli.
	(5)	Gradual prolongation of subjective response to touch and pain stimuli.
	(6)	Occasional "reference" of sensation, usually proximally.
20–25 mins.	(1)	Touch and position sense fail.
	(2)	Cold and warm sensibility may fail.
	(3)	Identification of temperature stimuli affected by repeated testing.
	(4)	Delay, prolongation and altered quality clearly apparent in all sensory reponses which can still be elicited.
	(5)	Paradoxical temperature sensations.
25–30 mins.	(1)	Cold and warm usually fail.
	(2)	Response to painful stimulus grossly altered in quality and duration: occasional failure to respond.
	(3)	Complete motor paralysis.
30–35 mins.	(1)	Warm sensibility may temporarily recover; grossly delayed and altered in quality.
	(2)	Pain may fail.
	(3)	The position of the limb in space may be felt subjectively to have altered, e.g. it may be difficult to convince the subject that his elbow has not spontaneously become flexed.
35–40 mins.	(1)	Warm stimuli frequently identified correctly.
	(2)	Pain usually fails.
RELEASE:	(1)	Often followed by evanescent spontaneous sensations of warm and cold.
	(2)	Pricking paraesthesia in limb is invariable and lasts for varying period.
	(3)	Sensory testing extremely difficult during paræsthesia: mode of recovery thus difficult to assess.

TABLE III

SENSORY PHENOMENA OCCURRING DURING NERVE BLOCKS

PHENOMENON	*Type of block*			
	Compression	*Procaine*	*Cold*	
1. "Spontaneous" sensations referred to distribution of nerve.	xxx	xx	x	"Tingling" characteristic of compression and procaine blocks. Occasional pain in all three types of block.
2. Increase in intensity of sensation.	x	x	x	Usually temperature stimuli (control not always tested).
3. Delay in appreciation of stimulus.	xxx	xx	xx	Most marked with temperature stimuli.
4. Prolongation of subjective response.	xxx	xx	x	Most marked with painful stimuli.
5. Alteration in subjective quality of touch and pain.	xxx	xx	xx	Most marked with painful stimuli.
6. Paradoxical temperature sensations.	xxx	xxx	xxx	
7. Alteration in response on repetition of stimulus.	xx	xx	xx	Temperature stimuli only.
8. Reference of sensation.	x			
9. Spontaneous temporary recovery.	x		xx	In compression blocks only involves temperature: in cold blocks all modalities involved. Not always tested in compression.
10. Sensory hallucinations regarding position of limb.	x			Usually related to elbow joint.
11. "Spontaneous" subjective sensation referred to distribution of nerve during recovery.	xxx		x	

x = Occasional.
xx = Frequent.
xxx = Characteristic.

Sensations induced by the blocking process.

The most characteristic of such sensations is a tactile paræsthesia occurring soon after the beginning of the block and felt in the territory of the blocked nerve. The occurrence of "tingling" or "formication" during asphyxial blocks of a limb or of a nerve has been noted by several observers (Bastien & Vulpian, 1855; Weir Mitchell,

1872; Dessoir, 1892; Bourguignon & Laugier, 1923; Bazett & McGlone, 1931; Hyndman & Wolkin, 1941; Kugelberg, 1944, 1946a, 1946b, 1948a, 1948b; Merrington & Nathan, 1949), and in 1947 Weddell & Sinclair investigated the nature of this sensation. It is curious that Reid (1931) and Lewis, Pickering & Rothschild (1931), who worked extensively with compression block of the upper limb, should not mention the occurrence of tingling: our own observations suggest that it is almost invariable when the compression cuff encircles the arm. When, however, the cuff is sited on the forearm, it is less effective (Weddell & Sinclair, 1947; Sinclair, 1948; Magladery, McDougal & Stoll, 1950) and tingling does not as a rule appear. Further, in order to elicit tingling of this type by direct pressure applied to a nerve trunk, it is necessary to exert the pressure over a certain minimal length of nerve (Weddell & Sinclair, 1947).

The sensation has a distinctive quality, and the various descriptions offered are very similar. Thus, Kugelberg (1946) calls it "a soft vibrating sensation"; Weddell & Sinclair (1947) refer to "a faint comfortable soda-water sensation," and Merrington & Nathan (1949) describe it as "a soft diffuse tingling." These descriptions closely resemble those of the sensations produced by weak electrical stimulation of the skin or of a sensory nerve trunk (Ebbecke, 1922; Crook, 1932; Thompson, 1934, and others). Indeed it appears that in compression blocks the sensation has its origin in the nerve trunks at the point where they lie under the cuff, and that the operative mechanism for firing them off is one of asphyxia (Weddell & Sinclair, 1947). It is perhaps relevant that Thompson (1934) has shown that tingling induced by electrical stimulation does not depend on the retrograde stimulation of end-organs, and that Lehmann (1937), produced spontaneous discharges in mammalian nerve by asphyxiation with nitrogen after a time lag in reasonable agreement with that of the tingling in cuff experiments.

Introspectively, the sensation appears to be a distortion of touch sensibility, with little suggestion of a painful component, and this view is supported by the experiments of Kugelberg (1946b), and those of Weddell & Sinclair (1947), who found that in four subjects with partial or complete loss of pain sensibility in one upper limb there was little difference in the sensation of tingling produced by cuff compression on the two arms.

Since then, thanks to the kindness of Mr. H. J. Seddon, Mr. G. F. Rowbotham and Dr. Ritchie Russell, one of us (D.S.) has been able to examine 28 patients with lesions affecting the touch or pain pathways on one side of the body. Among these patients was one in whom neurological evidence suggested that the posterior columns of the spinal cord had been damaged; two in whom antero-lateral cordotomy had been carried out; three in which wounds involved the thalamic region; one with thrombosis of the posterior inferior cerebellar artery, and 21 in which wounds involved various areas of the cerebral cortex, resulting in various degrees of disturbance of sensory appreciation. The results suggest that the impulses producing the sensation of tingling are mainly carried in channels other than those subserving pain sensibility and probably travel with touch impulses. Hyndman & Wolkin (1941) noted the occurrence of tingling sensations in response to the inflation of a compression cuff on the leg of a patient in whom the somatic nerves to the lower extremities had been interrupted, and suggested that this tingling might be carried in sympathetic sensory fibres; tingling of this type can, however, occur in a sympathectomized limb (Weddell & Sinclair, 1947).

In the cortical lesions, if there was complete anæsthesia, tingling could not be elicited by a compression cuff; if there was a degree of hypoæsthesia, perhaps amounting only to faulty perception of figure-writing or a reduction in two-point discrimination,

tingling was also diminished, or more commonly absent. If there was no sign of disturbance of touch sensibility, in general the tingling was "normal," though in two patients in whom ordinary testing indicated normal touch sensibility tingling was reduced or absent. The areas of cortex concerned with the impulses of this tingling may therefore be the same as those concerned with the sensation of touch, and cerebral defects in the appreciation of touch are possibly always accompanied by a defect in appreciation of the asphyxial tingling. It may even be that a diminution in the sensation of tingling affords a more sensitive test for such defects than the standard testing methods.

While the occurrence of tingling paræsthesia during asphyxial blocking of a nerve has previously been the subject of some comment, there is very little literature on the tingling which frequently occurs in procaine nerve blocks shortly after the introduction of the anæsthetic. This tingling appears, from the comments of those subjects who underwent both types of block, to be qualitatively indistinguishable from the asphyxial tingling, but is of much shorter duration, as would be expected.

Waller (1861) describes as a well-known fact the occurrence of pain in the ulnar area in the initial stages of blocks induced by immersing the elbow in ice-water. Boycott (1902), who immersed his elbow in a freezing mixture, reports "very marked sensory phenomena . . . further prosecution of the experiment became highly undesirable." From this it may be deduced that the phenomena were of a painful nature, but no further description is given. Wolf & Hardy (1941) report the occurrence of "pins and needles" on immersing the whole hand in water below 12° C.; the time relationships of this sensation are similar to those of the sensation induced by an asphyxial block, but the sensation itself appears to be qualitatively different, since it was unpleasant, and on one occasion at least led to withdrawal of the hand; they conclude from their experiments that this sensation is carried in the C fibres. On the other hand, Granit & Lundberg (1947) have shown that in cats cooling of thin strands of nerve-root filaments leads to "a selective large fibre response" more evident in sensory fibres. In the present series of cold blocks we have not observed any tingling or touch paræsthesia during the development of the block, but occasionally there has been a complaint of a deep, poorly localized, unpleasant pain in the region of the wrist. Similar pain has occasionally been noted in all three types of block, but in the other two it is much less constant than the tingling. It is perhaps curious that tingling was not observed in the cold blocks, since, as Weir Mitchell (1872) has observed, "When we touch or otherwise disturb any part of the trunk of a nerve containing sensory filaments we cause, if the irritation be feeble, subjective referred sensations, such as formication." The absence of tingling in cold blocks and its consistent presence in compression blocks suggests that the physiological mechanisms of the two types of block are probably not identical—a suggestion which we have already put forward on other grounds (Sinclair & Hinshaw, 1951b).

Alterations in the response to sensory stimulation.

1. *Increase in intensity of sensation.*

In the early stages of some of the compression blocks the subjects remarked on what they thought was the introduction of a different set of temperature stimuli, since they now felt the warm stimulus had become much warmer and the cold stimulus colder than at the beginning of the experiment. On investigation it was found that this occasionally occurred in all three types of block, and an attempt was made to compare the sensation produced on the blocked hand with the sensation produced by the same stimulus on the control hand. It was found that in certain cases the warm

stimulus was consistently called "warmer" on the blocked hand than on the control, and similarly the cold stimulus appeared colder on the blocked hand than on the control. The stimuli used naturally permitted only a rough assessment to be made, and no measurement of thresholds was attempted. We could not satisfy ourselves that a similar "hyperæsthesia" occurred in modalities other than that of temperature, and even with temperature stimuli the effect often lasted for only a few minutes in the early stages of the block. Brown-Séquard (1857) found an initial hyperæsthesia in the limbs of rabbits deprived of their blood-supply, and Waller (1861) noted a hyperæsthesia at the beginning of nerve blocks induced by cold in man. Again, Goldscheider (1886) concluded that there is a transitory lowering of the pain threshold during the initial stages of the development of surface anæsthesia with cocaine. On the other hand, Dessoir (1892) reported a hypoæsthesia and hypoalgesia in the early stages of compression blocks.

In 1936 Thompson and Kimball reported a drop in the threshold alternating current necessary to evoke sensation from the median, ulnar and radial nerves at the wrist during the first 12 minutes of a compression block of the upper limb. Whether this lowering in threshold has any connection with the subjective increase in the intensity of the sensation occasioned by a constant stimulus is a matter which requires further experiment.

2. *Partial block syndrome.*

At a certain stage of compression cuff blocks, a characteristic picture develops which might be called the partial block syndrome. The response to a touch or pain stimulus is delayed, prolonged and altered in quality, while temperature stimuli evoke a response which is delayed, often prolonged, and may be paradoxical in nature (Sinclair, 1948; Weddell, Sinclair & Feindel, 1948; Sinclair & Hinshaw, 1951a). The fully developed syndrome is perhaps most striking in the modality of pain. Thus,

TABLE IV

Alterations in Responses to Stimulation: All Types of Block

Modality	*Delay*	*Prolongation*	*Alterations in quality*
1. Touch ..	1–2 seconds	2–5 seconds	Feels "dull" and "flat," lacking "overtones." Alterations indistinguishable with different blocking agents.
2. Pain	2–5 seconds*	5–10 seconds	Severe unpleasant affective reaction in cuff-blocks; neither so severe nor so frequently obtained in procaine and cold blocks.
3. Temperature	5–10 seconds	?	Quality changes detectable, specially in cold blocks, but in all blocks overshadowed by almost invariable occurrence of paradoxical sensations.

The figures given are rough approximations only and are not based on exact measurements. They indicate the maximum times encountered. The prolongation of temperature sensations, though probably a real phenomenon, proved difficult to estimate under the conditions of sensory testing.

* Delay longest in compression blocks.

the subject may say of a pinprick, "That is sharp and feels as though it ought to be painful"; perhaps three seconds later his face becomes contorted, his pupil dilates, his hand is involuntarily withdrawn, to the accompaniment of an exclamation, and sweat may appear on his forehead and round his mouth. He now reports a particularly unpleasant experience: a slow swelling burning sting, which may go on and on for several seconds, not very intense, but of a character rendering him fearful of further such experiences. On being pressed, he will find himself unable to compare the sensation with anything he has experienced previously, but may use the analogy of a wasp-sting, or a very fine-pointed, red-hot needle being slowly inserted. It should perhaps be emphasized that the fully developed response does not invariably appear with every stimulus; thus, testing within a given circle at a given time may produce a relatively normal response, a delayed normal response, a prolonged normal response, or a response which is both delayed and prolonged, but not altered in quality. At the same time there may be immediate responses of abnormal quality, prolonged abnormal responses, and finally the characteristic response, delayed, prolonged, and of abnormal quality.

The three components of this sensory syndrome do not always obtrude themselves in the same sequence, nor are they of comparable severity in all modalities (*see* Table IV). Thus, the alteration in quality of touch sensibility is unobtrusive, and may occasionally be missed, whereas no subject experiencing the alteration in the quality of pain in its fully developed form could conceivably overlook it.

A very similar picture is found during the development of both procaine and cold blocks (Table IV), the only major difference being in the relative frequency and severity of the unpleasant response to pinprick (Sinclair & Hinshaw, 1951a and 1951b).

All three sensory symptoms have been the subject of comment in the past, but it is not usual to find them considered in relationship to each other.

A. Delay.

A delay between the application of a stimulus and its recognition has often been noted in compression or pressure blocks (Fabritius & Bermann, 1913; Lewis, Pickering & Rothschild, 1931; Zotterman, 1933; Lewis & Pochin, 1937, 1938; Wortis, Stein & Joliffe, 1942; Sinclair, 1948; Weddell, Sinclair & Feindel, 1948), but in procaine and cold blocks its occurrence and possible significance appear to have escaped any extensive notice.

Zotterman (1933) measured the delay in the perception of stimuli applied to the hairs, and found a steep rise in this time about the 18th minute of a cuff compression. More important, because of the conclusions which have been drawn from them, are the measurements made by Lewis and Pochin (1937) on the delay in pain perception. Impressed by the existence of a double pain response to a single prick on the normal finger, they measured the interval from the stimulus to the beginning of the second response (11 readings in two subjects), and compared these control figures with the interval from the stimulus to the beginning of the delayed response in the same fingers during a cuff block (12 readings in the same subjects between about the 25th and the 30th minute of the blocks). The method of measurement referred to indicates a heat stimulus, but the text and the table imply a pinprick stimulus; no method of measurement is given for the latter. Finding the average of all control readings (about 1·2 seconds) to be almost the same as the average of all the asphyxia readings, they identify the delayed response in asphyxia with the normal "second response." It may be noted, however, that when each subject is considered separately, the figures given show a statistically significant difference in both cases between the control and the asphyxia readings. Further, in these experiments no account is taken

of possible alterations in the "reaction time" during asphyxia. In a second paper (Lewis & Pochin, 1938) they state (without offering any additional measurements) that in cuff experiments the pain response "is delayed by at at most 1·5 seconds. . . ." This statement later develops into ". . . for pain the latency rises abruptly from 0·3 to 1·5 seconds, where it remains constant." They contrast this behaviour with that of the cold response, in which they describe a gradually increasing delay to values of up to 10 seconds, and conclude "Clearly the cold response, unlike the pain response, is not carried by two groups of nerve fibres of widely differing conduction rates." In the absence of any figures supporting the asserted "sharp rise in latency" in the case of pain, or the "constant" value attained it is difficult to comment on the validity of these conclusions; the inadequacy of the published measurements needs no emphasis. Wortis, Stein & Joliffe (1942) timed with a stop-watch the delay in the response to pinprick on the foot at intervals during cuff experiments, and obtained values of from 0·9 to 1·7 seconds. They stress the inaccuracy of the method, but support the conclusions of Lewis & Pochin. We have made no attempt to measure the average delay at different stages of compression blocks with any accuracy, but on numerous occasions have recorded (with a stop-watch) delays much in excess of 1·5 seconds, and indeed up to 5 seconds—longer than any fibre currently stigmatized as carrying pain would take to conduct an impulse from the finger to consciousness. (Like those of Lewis & Pochin, these figures do not differentiate between delay due to prolongation of the "reaction time" and delay due to an increase of the time necessary for the evaluation of the nature and quality of the stimulus).

Gasser (1943, 1945) is of the opinion that each modality of sensation is widely distributed throughout the range of fibre sizes, and our own sensory dissociation experiments have led to substantially the same conclusion (Sinclair & Hinshaw, 1951a and 1951b). Further, Magladery, McDougal & Stoll (1950) have recently recorded sensory potentials from the ulnar nerve at the elbow during cuff blocks, and find that "When latencies and amplitudes were plotted against duration of ischemia, there was no break in the smooth curve obtained. It would seem reasonable to assume that this implies a progressive depression and block in the same fibres which initially conducted impulses of high velocity . . . it is possible . . . that ischemia . . . creates a more synchronous volley in the slowly conducting group. . . ."

The findings in procaine and cold blocks are also relevant. The electrophysiological evidence suggests that in procaine blocks the small slowly conducting fibres fail before the large ones, and in cold blocks it appears that the fibres fall out in an order independent of their conduction rates (Bickford, 1939; Sinclair & Hinshaw, 1951b). Yet in both types of block considerable delays have been recorded in the modality of pain, though less frequently than in compression blocks (Table IV). In summary, it may be said that the evidence is at present inadequate to support the identification of the delay between a pain stimulus and "second pain" in normal skin with the delay in asphyxial blocks. It follows that the cause of the asphyxial delay is not necessarily the same as the cause giving rise to the appearance of "second pain," and that it might be profitable to look for a mechanism capable of explaining the occurrence of delay in all modalities in all types of block.

Weddell, Sinclair & Feindel (1948) have already suggested that the delay in the appreciation of painful stimuli in cuff experiments and in clinical conditions such as tabes (Naunyn, 1879; Gad & Goldscheider, 1892; Pochin, 1938; Kendall, 1939; Stein & Wortis, 1941; Kugelberg, 1948; Ashby, 1949, and others) does not necessarily depend on the existence of two discrete groups of fibres conducting at different rates. They point out that the delay could occur, not in the peripheral nerve, but in the synapses of the cord, or in the higher centres, as a result of a simplification of the

impulse pattern reaching the central nervous system. Such an explanation would obviously be applicable to all modalities and to all types of block. It is interesting that a similar central factor has been postulated by Stein & von Weizsäcker (1927) to account for the delay in perception of stimuli in clinical cases of hypoæsthesia, and by Kauffmann & Steinhausen (1921) and Piéron (1930) to explain the observation that the reaction time to pain in normal subjects varies with stimulus intensity (Dolley & Cattell, 1894; Kauffmann, 1921; Piéron, 1929; Eichler, 1930; Piéron & Segal, 1938). It is possible that a similar mechanism underlies the variation in the reaction time to pain when different kinds of stimuli are used (Piéron, 1929, 1930, 1931).

Pochin (1938) attempted to show that the delay in pain perception occurring in tabes is the same as the delay in perception of "second pain" in normal subjects, and found the delay to be similar in both instances. The results may, however, be criticized on the grounds that for the tabetics he used a needle with "a 20 g. thrust" and for the normal controls one with "a 2 g. thrust." In any case, as Ashby (1949) points out, there are many recorded instances of much longer delays in tabes, some of which are too long to be accounted for satisfactorily on the basis of delayed nerve conduction alone.

B. Prolongation of response.

Prolongation of the delayed response to a pain stimulus is well recognized in both clinical conditions and nerve blocking experiments (Naunyn, 1879; Stein & von Weizsäcker, 1927; Zotterman, 1933; Lewis & Pochin, 1938; Kendall, 1939; Wortis, Stein & Joliffe, 1942; Weddell, Sinclair & Feindel, 1948; Ashby, 1949, and others). In our experience with nerve blocks, the response to a painful stimulus has frequently lasted as long as 10 seconds, though there is considerable variation, and the time does not appear to increase regularly with the progress of the block. In clinical cases, too, the response can often be of the order of 10 seconds in length (Ashby, 1949). In spite of its frequent occurrence, however, no adequate explanation for the existence of this prolongation has been put forward.

Sinclair (1948) noted a considerable prolongation of the sensation aroused by touch stimuli at a certain stage of progress of compression blocks, and we have since found this to be a fairly consistent feature of procaine and cold blocks also (Table IV). The methods of testing (Sinclair & Hinshaw, 1950) do not permit a categorical statement that the same thing occurs with temperature stimuli, but our impression is that it probably does. At all events, since touch has not yet been associated with two special groups of fibres, we are at liberty to consider the meaning of the phenomenon without the necessity of binding it to the process of conduction in a specific group of nerve fibres, as has hitherto been the case (Lewis & Pochin, 1938; Kendall, 1939, and others). It is perhaps possible that the prolongation of the response is related to the reduction in the number of impulses reaching the central nervous system. Though the analogy must not be pushed too far, it is interesting to speculate whether this type of response is the sensory counterpart of the long "after-discharge" which often occurs in motor reflexes when the stimulus is brief and weak (Sherrington, 1947, p. 33).

C. Alterations in quality of response.

It has already been noted that alterations in quality in asphyxial block experiments are most marked in the modality of pain, and it is to pain that nearly all the attention in the literature is directed (Bastien & Vulpian, 1855; Boeri & di Silvestro, 1899; Fabritius & Bermann, 1913; Lewis, Pickering & Rothschild, 1931; Zotterman, 1933; Lewis & Pochin, 1938; Wortis, Stein & Joliffe, 1942; Weddell, Sinclair & Feindel, 1948; Kellgren & McGowan, 1948, and others). The close resemblance between this type

of distortion of pain sensibility and the distortion which occurs in clinical conditions such as nerve regeneration (Head, 1920; Trotter & Davies, 1913; Lanier, 1935, and others), and tabes (Naunyn, 1879; Ashby, 1949, and others), has been noted by many workers, and the matter has been recently discussed by Weddell, Sinclair & Feindel (1948), in the light of observations on the neurohistology of cutaneous scars from which unpleasant pain could be elicited. It was found that the occurrence of an unpleasant response to a needle prick appeared to depend on the existence of a simplified pattern of innervation in the scar, and this was correlated with the occurrence of a very similar type of pain in compression blocks at a stage when the fibres supplying the area tested must be assumed to be falling out randomly and so thinning the pattern of innervation. The idea put forward, which owed much to the suggestions of Symonds (1931), Lanier (1935) and Bishop (1946) was that the unpleasant quality of pain sensibility was correlated with a reduction in the pattern of impulses reaching the central nervous system as a result of a stimulus.

It is interesting, however, and possibly important, that in both procaine and cold blocks the alteration in the quality of pain sensibility is not nearly so marked as it is in compression blocks; indeed, in some cases it was not observed (Tables II and IV), in contrast to its invariable appearance in compression blocks. In all three types of blocks the available evidence suggests that the fibres conducting any one modality from a given area of skin tend to fall out at random, so producing a gradually diminishing pattern of innervation in the skin. A non-specific reduction in neural patterns does not, therefore, appear to be a sufficient explanation for the occurrence of unpleasant pain, and it is probable that a special *kind* of reduction is necessary. We have suggested elsewhere (Sinclair & Hinshaw, 1951a) that the explanation of the differences between compression blocks and procaine or cold blocks may lie in the different affective reaction produced by the activity of large and small fibres. In compression blocks it is possible that the larger fibres are attacked first; these fibres may simply be responsible for the recognition by the subject that he has been pricked, and have little relation to the sensation of pain as it is commonly understood (Trotter & Davies, 1913; Le Dantec, 1919; Piéron, 1923). The remaining smaller fibres, which give the subject the impulse pattern commonly identified by him as "pain," are therefore those which are left to undergo the terminal "thinning out" process which governs the appearance of the unpleasant response. In procaine and cold blocks it is likely that the fibres taking part in this "thinning out" process are, at least in part, the larger fibres subserving "piqûre" (Piéron) and not pain; a reduction in pattern of such a fibre network would indeed give rise to a change in quality, but such a change would not necessarily have the explosive, unpleasant quality of a change in quality aroused within the range of "true" pain.

Any explanation of this sort would, of course, also account for the occurrence of alterations in the quality of other modalities, such as touch (Sinclair, 1948). In all three types of block alterations in the quality of temperature sensibility occur, but they are more difficult to define, and may best be described as giving the impression that the sensation has lost its "brightness" and is descending gradually on the skin from an immense distance; it "soaks in" in a manner quite unlike its normal brisk progress through the skin, and the process is obtrusive. Several subjects were still able to differentiate a warm stimulus from a cold one at a time when both sensations had apparently lost all temperature content (Sinclair & Hinshaw, 1950). No subject could say how this was done, except that the tubes "felt different"; they could not identify either tube as being warm or cold, but could merely distinguish tube A from tube B. Their ability in this respect did not alter when the two tubes were exchanged for others.

D. Paradoxical temperature sensations.

Alterations in quality of temperature sensation were in all cases overshadowed by the occurrence of paradoxical sensations, which were characteristic of all three types of block. In the literature, mention of paradoxical temperature sensations during nerve block experiments is practically restricted to chemical blocks (Ponzo, 1909; Schilder, 1913; Goldscheider & Hahn, 1924; Grundig, 1930). Their occurrence in clinical conditions such as nerve injury has also been noted (Barker, 1896), though such reports are relatively uncommon, perhaps because the patient has too often been written off as "unreliable" by the clinician.

Previous workers on the normal skin have tended to stress the phenomenon of paradoxical cold as opposed to paradoxical warm (Alrutz, 1897, 1900, 1906; Thunberg, 1901, and others), but, as Pavlicek & Jenkins (1933) point out, paradoxical warm is commoner than has often been supposed (Goldscheider, 1884; Bazett, McGlone & Brocklehurst, 1930). Nevertheless, Jenkins (1938) using a stimulator surrounded by a variable temperature field, observed paradoxical cold and warm only 27 times in 9,000 stimuli, and in only two out of four subjects, and Gilmer (1942) notes only one report of paradoxical warm from approximately 13,000 cold stimuli. However, in blocking experiments Ponzo (1909) and Schilder (1913) describe paradoxical warm occurring after the injection of stovain into the skin, and Goldscheider & Hahn (1924), who applied various chemicals to the surface of the skin, maintain that paradoxical warm is just as common under these circumstances as paradoxical cold. On the other hand, Grundig (1930) blocked the anterior cutaneous nerve of the thigh with tutocain, and could find no evidence of the occurrence of paradoxical warm from the warm spots in the interval after cold sensibility had "disappeared" while that of warmth was still present. He agrees with Rein (1924) that there is insufficient evidence for the existence of physiological paradoxical warm.

There is thus considerable disagreement in the literature, and it should perhaps be emphasized here that the phenomenon we have described is the appearance of a paradoxical sensation in response to a relatively large stimulus. Blackening the end of the test-tubes used showed that the average area of the tube in contact with the skin was about 50 sq. mm., whereas most of the preceding work has been done with small stimuli designed to investigate the responses of individual cold and warm "spots." One would not, therefore, expect the results to be strictly comparable. With our method of stimulation, there are periods during the development of practically every block when the cold stimulus is called "warm" and the warm stimulus called "cold"; both phenomena are equally common. It is also quite usual to find that in one and the same area at the same time cold is called warm and warm is called cold. This finding would appear to have a number of implications bearing on current theories of paradoxical temperature sensation (Stone & Jenkins, 1940), and on the problem of temperature sensitivity as a whole. One of us (J. R. H.) has a paper on the subject in preparation.

There were additional complications. In all types of block it was quite common to find that, during the failure of temperature sensibility, the subject might confidently call a warm stimulus cold immediately after its application. After a few seconds he might revise his opinion and call it "indifferent," and by about the 10th second he might be equally confident that the stimulus was a warm one. This opinion did not alter if the stimulus was left *in situ* for as much as another 10 seconds. On removal of the stimulus, however, the subject might once again reverse his opinion and maintain that the stimulus was cold after all. Such rapid alterations of subjective judgment were, of course, not invariable, but they were observed in many subjects

with both cold and warm stimuli; they did not appear to be a reflection on the reliability of the individual subject.

The effect of repetition of temperature stimuli on the response obtained was also observed (Sinclair & Hinshaw, 1950). Frequently a stimulus which elicited a paradoxical or an indifferent response on first application would be recognized correctly and consistently after it had been applied several times to the same area.

3. *Localizability of stimuli and reference of sensation.*

During the process of extinction of the modalities of touch and temperature in compression blocks, there were occasional spontaneous complaints from the subjects that the stimuli were being applied to a different area, sometimes on the hand, sometimes on the forearm or elbow. These "references" of touch and temperature sensations were inconstant, and were not noticed in either procaine or cold blocks. The referred sensation usually had an altered quality and felt, in the case of touch, like a prolonged "tickle." Often stimulation of the same area a few minutes later would still produce reference to the same situation. It is perhaps curious that reference of pain was not observed, since pain is fairly frequently referred from the normal skin (Sinclair, 1949). Further, painful stimuli remained fairly well localized even at the stage of unpleasant responses, whereas touch and temperature stimuli were often poorly localized just before they disappeared. Little can as yet be said regarding the reason for the occurrence of reference in compression blocks; it may perhaps be connected with the progressive reduction in sensibility of the stimulus area, which would render a referred sensation more noticeable than under normal circumstances, when the impression from the stimulus area would be so overwhelming as to exclude from attention the feeble referred sensation. This sort of explanation might also account for the lack of reference of pain at a state when the local response is characteristically unpleasant.

4. *Extinction of sensibility.*

The main object of carrying out the blocks reported in this paper was to throw light on the factors determining the order in which the various sensory modalities apparently disappeared in such experiments. This work has been reported elsewhere (Sinclair & Hinshaw, 1950, 1951a and 1951b), but a number of points of interest may be stressed.

The extinction of a given modality is a gradually progressive and continuous process; when one spot ceases to respond, another close by may continue to give responses for some time, and the pattern formed by such responsive spots gradually becomes thinned out and disappears. When responses to a given stimulus can no longer be obtained, a more intense stimulus—e.g. a heavier nylon thread—will still produce responses, and when these in turn disappear, a further increase in stimulus intensity will prolong the extinction time still further, and so on. Repetition of touch stimuli, individually ineffective, often leads to a response, probably owing to some sort of central summation process (*cf.* Achelis, 1936). It is essential, therefore, in this type of work, to specify exactly the nature of the stimulus and its mode of application before it becomes possible to say that "touch" disappears before "pain" and other statements of the kind. Too little regard for this necessity has, in the past, lead to grossly contradictory results, since by manipulation of the stimuli used it is often possible to record several different orders of sensory loss in the same experiment. Further, the end-points adopted must be the subject of close definition. In this work we used end-points of the "50 per cent. mortality" variety, in the belief that the type of response curve under these circumstances probably corresponds to the familiar

dosage/mortality curves of probit analysis. Unfortunately the rapid progress of the blocks did not permit us to base our 50 per cent. mortality on more than 6 or 8 successive stimuli. Nevertheless, the endpoint so defined is capable of fairly accurate reproduction by different observers, and in view of the many other factors involved, is probably accurate enough for the purpose. Thompson, *et al.* (1934), dealing with a similar problem, used a nil response from three *consecutive* stimuli as a criterion of a stimulus being below threshold, and justify their method mathematically; it is probable that this system is preferable.

We have no measure of the absolute energies involved in the stimuli used in this investigation, and it is likely that they differ grossly from one modality to another. For this reason, the end-point for one modality is almost certainly not comparable to that of another, in terms of the nerve impulses aroused by each stimulus. Again, the results obtained in one area of skin within the territory of a given nerve can be different from those in another area, and the behaviour of one nerve territory can be grossly different from that of another. In short, it appears unlikely that it will ever be possible to say, in absolute terms and without numerous qualifications, that in nerve blocks in man one modality fails before another. Any inferences drawn from such statements, such as a relation between fibre size and sensory modality (Gasser & Erlanger, 1929), are therefore open to question.

5. *Spontaneous temporary recovery.*

In the cold blocks in this series it was frequently observed that there were occasional spontaneous remissions in the march of the sensory paralysis, during which the sensibility of the part might recover to a condition indistinguishable from normal by the testing methods at our disposal. Several such recoveries might occur during the course of one block, without any alteration in the temperature of the freezing mixture. This type of irregular progress of paralysis in cold blocks appears to have been first noticed by Waller (1861) who noted that in some cases "we have hyperæsthesia and analgesia of the nerve recurring several times before the supervention of the stage of anæsthesia." Lewis (1941) has described a related type of irregular recovery in sympathetic fibres under similar circumstances.

Perhaps more important, because of its bearing on the nature of the modality of warm, is another type of sensory recovery affecting only this modality and occurring both in cold blocks (Sinclair & Hinshaw, 1951b) and in compression blocks. In this case the reappearance of an ability to identify warm stimuli correctly and consistently some time after this ability had been extinguished appears to be connected with the onset of vasomotor paralysis. In the cold blocks, at any rate, it is strongly correlated with the appearance of a rise in skin temperature, and during the period of recovery it may disappear temporarily with the recovery of vasomotor control (*see* p. 67). In procaine blocks, vasomotor paralysis occurs too early (it is often the first sign) for the connection to be investigated.

It is also interesting that there is, in both cold and compression blocks, frequently a sensation that the whole hand is warm and glowing, at a stage when no responses occur to warm stimuli applied to the skin. Lewis, Pickering & Rothschild (1931) note that in compression blocks, as late as the 35th minute, the tip of a finger dipped in water at 40° C. "feels an undiminished and glowing warmth."

There have been numerous attempts to correlate the sensations of warmth and cold with the activity of blood vessels and the sympathetic nervous system generally (Nafe & Wagoner, 1936; Hyndman & Wolkin, 1941; Gilmer, 1942, and others) and certainly no satisfactory specific end-organ has ever been produced to bear out the theory suggested by v. Frey (Dallenbach, 1927, 1929; Kantor, 1947, and many

others). Nafe & Wagoner (1937) found that in compression blocks the failure of cold sensibility coincides with the failure of the blood-vessels in the hand to respond to a cold stimulus given to the contralateral hand. Their conclusions do not appear justified by the few observations presented, but nevertheless it appears that work of this kind could throw some light on temperature sensibility. The findings in the present series of blocks have not yet been followed up, but they suggest a possible line of enquiry.

Sensory hallucinations.

These were confined to compression blocks, and related to the position of the elbow. Seven subjects, at a time when all sensation in the hand except that of pain had been extinguished, spontaneously remarked on the fact that they felt that the forearm had risen from its dependent position and now lay roughly at right-angles to the arm. All were profoundly surprised to find that in fact the forearm had not moved. After they had satisfied themselves of this, the hallucination usually disappeared.

Process of recovery.

1. *Sensations induced by the recovery process.*

No particular sensation could be associated with the recovery process in procaine blocks, but in a number of cold blocks recovery was accompanied by pain in the cooled region of the elbow, with or without nausea and faintness increased by handling the territory of the ulnar nerve. This may be due to the liberation and rapid dissemination of a histamine-like substance from the damaged tissues of the elbow, and it was found that if the limb was kept quite still and not interfered with, these symptoms did not occur (Sinclair & Hinshaw, 1951b). Pain referred to the hand was not observed.

On a release of a compression block, a series of fleeting thermal impressions was often felt, such as a "wave" of warmth appearing to "hit" the hand, succeeded by a transient feeling of cold lasting a few seconds. This was in turn followed by a warm "glow." These temperature sensations were inconstant, and have not been investigated.

The existence of "formication" during the recovery from compression blocks has been noted in the literature for some time (e.g. Carpenter, 1847), but it was not until 1931 that Lewis, Pickering and Rothschild investigated its nature and cause. They concluded that it arose in the nerve fibres directly underneath the investing cuff, and suggested that it was "an affair of the tactile nerves." Weddell & Sinclair (1947) described several elements in the sensation complex which succeeds release of compression, and concluded that the tingling or "pricking" component was largely due to the stimulation of pain endings in the hand by the removal from their vicinity of a metabolite which had accumulated there during the period of compression.

Merrington & Nathan (1949), as a result of similar experiments, concluded that the impulses arose in the nerves and not in the endings, and that they were carried in fibres normally subserving touch, pressure and movement.

This conflict regarding the quality of the sensation produced in this way results largely from the observation of patients with lesions of the nervous system, which is necessarily an unreliable basis for work on sensation. One of us (D. S.) has investigated release paræsthesiæ in the same 28 patients used for the investigation of tingling during compression. The evidence so accumulated suggests, in contrast to the findings of Merrington & Nathan, that the impulses of release paræsthesiæ travel, at least as far as the thalamus, along pathways normally conveying pain; when these

pathways are intact, the sensation is felt, and when they are damaged, the sensation is diminished or absent. In the cases with cerebral lesions, defects in the appreciation of pain were correlated with defective appreciation of the paræsthesiæ, while defects in the appreciation of touch or pressure produced little or no effect on the intensity, duration or quality of the sensation.

The most recent work is that of Magladery, McDougall & Stoll (1950), who recorded the impulses from the ulnar nerve at the elbow during and after compression blocks. Their evidence indicates that the impulses arise distally in the limb, probably at the endings, and they have shown that the first fibres to recover (about the time of onset of formication) are those of high conduction velocity. They conclude that pain pathways are not likely to be involved.

These difficulties may perhaps be resolved by a consideration of the introspective findings. When a subject is asked to describe the sensation he experiences under these circumstances, one of two replies is almost invariably made; either it is called "pins and needles" or it is described as a "pricking" sensation. The implication of "pricking" is thus invariable, though the sensation may be described as painful by one subject and not painful by another. May it not therefore be that the sensation is closely connected to the sensation of "piqûre" which Piéron and others have stoutly maintained must be separated from pain? It would occasion no surprise to find "piqûre" travelling in the fast fibres, and indeed it could be regarded as the response typically aroused by the largest fibres in a spectrum whose slower members give responses which are characteristically recognized as painful. The pathways traversed by the sensation of "piqûre" may well be multiple, and may differ from patient to patient, but it is far more likely that confusion arises owing to the difficulty of patients in interpreting and naming the sensations which they experience.

2. *Response to sensory stimuli during recovery.*

In both cold and compression blocks the process of sensory recovery was usually extremely difficult to follow because of the risk of causing syncope in the cold blocks and the constant presence of paræsthesiæ in the compression blocks, so that the procaine blocks afford the only really satisfactory data on recovery. In the literature, records of the mode of recovery in compression blocks are scanty, though Raffel (1936) has reported two experiments on the leg. Recovery in cold blocks has been studied by Bickford (1939) and in procaine blocks by Heinbecker, Bishop & O'Leary (1934). With procaine blocks there is a difficulty not experienced in the other two types, namely, that the process of recovery is self-initiating, and no control of its onset is possible. Nevertheless, enough information has been accumulated in all three types of block to make certain general statements permissible.

Three stages could be recognized in the recovery process; the first being the ability to recognize the sensory stimuli consistently. This stage is reached quickest in compression blocks and slowest in procaine blocks; the various modalities do not always recover in the same order as they disappeared. In the cold blocks it was observed that the modality of warm, which was often present at the end of the block, disappeared again in several instances at the time of recovery of the sympathetic fibres, and only later returned permanently.

The next stage was reached when no subjective difference could be detected between the sensations produced by the standard stimuli on the affected and on the control hand; this took considerably longer than the first stage in all types of block. Though no difference could be detected by the crude stimuli employed, at this time there was still a subjective difference between one hand and another, and it was not until perhaps some hours later that the last stage—that of complete subjective

recovery—was reached. The difference between the two hands, which could not be satisfactorily described by any subject, could be emphasized by stimuli such as stroking the hand with the other hand. In all types of block the last modality to recover *completely* was that of touch, irrespective of the order in which touch sensibility returned when the standard stimuli were used. This finding may be correlated with the observation that during the development of the blocking process the first modality to be subjectively disturbed was in nearly every case that of touch, irrespective of the type of blocking agent and of the order in which the modalities eventually disappeared. We have previously (Sinclair & Hinshaw, 1951a) drawn attention to this discrepancy and suggested that a small deficiency in the modality of touch, a modality of which we make use so constantly, may be more readily detected than similar deficiencies in the other modalities, which we do not so often experience.

IV

General Discussion

Bastien & Vulpian (1855), who were among the first to conduct systematic investigations into the phenomena accompanying nerve blocks caused by the application of pressure, wrote:—"En résumé, une première et attentive exploration nous a fait voir que l'étude des effets de la compression des nerfs, de ces phénomènes très-connus, mais peu analysés jusqu'à présent, était une mine très riche et pouvait être féconde en résultats applicables à la physiologie et à la pathologie du système nerveux." The position is still very similar. The "mine" is still unexhausted, and the results still wait to be worked out. Even the phenomena themselves are by no means so generally known as was assumed nearly a hundred years ago, and it is partly for this reason that misconceptions have risen, such as the correlation of the order of sensory loss in nerve blocks in man with the way in which fibres fall out according to their size in animal experiments. In the human experiments insufficient attention has often been paid to controlling the mode of stimulation and the area stimulated and to the possibility of extensive individual variation.

One of the most interesting features of nerve blocks in general is the development of what we have called the "partial block syndrome"—i.e. the association of delay, prolongation of response and altered quality of response. It does not seem improbable that all three features of this syndrome may arise from the same basic cause—a deficiency (perhaps of a special kind) in the impulse pattern reaching the central nervous system as a result of the application of a stimulus. Such a hypothesis might well be applicable to similar phenomena known to occur in clinical lesions of the nervous system.

Similarly, the further elucidation of the precise nature of the sensations produced by the development and recession of the blocking process might have a direct application to problems connected with the occurrence or paræsthesiæ in nerve injuries, etc.

The phenomena in relation to temperature sensibility also merit further investigation. The paradoxical sensations, which are produced with difficulty from the normal skin, but which are so readily elicited by the technique described, may well throw light on the nature of temperature sensibility in general, while the possibility of a kind of relationship between warm sensibility and the activity of the sympathetic nervous system is also of importance. When so little is known for certain about temperature sensation, additional lines of attacking the problem have a disproportionate significance.

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VARIATIONS IN REFLEX BLINK-RATE DURING VISUAL-MOTOR TASKS

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The influence of the performance of various tasks on the reflex blink rate has been investigated repeatedly, but the results obtained have been somewhat contradictory. More recently, it has been suggested that the reflex blink rate may be a considerable factor in the accuracy with which visual-motor tasks are performed. The aim of the present experiment was to attempt to obtain further evidence on both these questions.

The experiment was carried out in two parts. The main part was done in the laboratory, while a validating experiment was carried out on the road. In the laboratory, subjects were required to steer a pencil along a moving track which varied in difficulty. The difficulty of the response was varied by using a direct control on some trials and a velocity control on others. Blink rate, errors, and control measurements were recorded throughout. In the road experiment, cine films were taken of the driver's eyes while driving in heavy traffic and in open country.

The results of both experiments show:

(*a*) That there are marked individual differences in blink rate, the relative order of which is maintained in spite of variations in the actual blink rate.

(*b*) That there is no relationship between the accuracy with which a particular individual carries out a task of this sort and his blink rate. The rapid "blinker" is no more and no less likely to be accurate than the infrequent "blinker."

(*c*) That the actual blink rate for all individuals varies inversely with the difficulty of the task and the amount of control movement necessary. The blink rate decreases as the necessity for detailed visual control of movement increases. Blink rates when driving a car in heavy traffic, or when steering along an oscillating track, are considerably lower than when driving in open country or when following a straight track.

(*d*) That the adjustment of the blink rate to the difficulty of the task is achieved not only by an alteration in overall blink rate, but also by a change in the distribution of blinking. The blink rate is approximately constant under constant conditions, but when the task is varying in difficulty, blinking occurs just before and just after periods of maximum difficulty, but is completely inhibited during the periods of maximum difficulty itself.

I

Introduction

The relationship of the reflex, involuntary blink rate to the type of activity in which the individual is engaged has been a subject of controversy for some years. Ponder and Kennedy (1928) attempted to discover the effective stimulus for the involuntary blink and were able to show that this was not provided by corneal or conjunctival stimulation, by the perception of light, or by tensions in the muscles controlling eye movements. They concluded that there appears to be no afferent path for this response, and that the response probably has a central rather than a peripheral origin. They suggested that the involuntary blink is caused by periodic discharges down the 7th nerve, the impulses originating in the basal nuclei. In the course of their experiments, they demonstrated that the blink rate varies between individuals, but is relatively constant for a given individual if the conditions are kept constant. Some

changes of condition, such as a change from light to darkness, did not affect the blink rate of their subjects significantly, while other conditions, such as changing from reading to conversation, from far to near fixation, sudden anger, and the subjection of witnesses in a court of law to cross-examination, produced marked changes, usually in the direction of increasing the blink rate. From comparative evidence supplied by Blount (1928), they suggest that in early life "blink movements develop either in connection with attempts at action on the part of the child, or with the perception of unexpected sounds or objects in its environment," and conclude that the blink movements are clearly linked up with mental development, so that "there is every reason to expect the close relationship which exists between the movements and the mental operations of later life."

That there is a marked and fairly consistent variation in blink rate between different inviduals has been demonstrated conclusively, and there appears to be little doubt that the blink rate for a given individual varies from time to time. The nature of this individual variation, however, does not seem to be at all clearly known. Peterson and Allison (1931) found that reading, cancelling letters, near and far fixation gave markedly different blink rates. Telford and Thompson (1933) recorded an increasing blink rate comparing reading, conversation and mental arithmetic in that order. Similar results were obtained by Walter (1941). Macpherson (1943), comparing blink rates using as tasks counting rows of dots, picking up aircraft silhouettes, plotting graphs and reading, concluded that rate of blinking is negatively correlated with the ease of doing a task.

While Luckiesh (1944 *a*) is sceptical of the use of the blink rate to equate different kinds of task, he and his collaborators (Luckiesh, 1944 *b*, 1946, 1947; Luckiesh & Moss, 1942; Luckiesh, Guth & Eastman, 1947), have claimed changes in the blink rate, on a given task, with changes in the relative ease of seeing. Using reading at a normal speed as a task, they have found increases in the blink rate during one hour, the rate of increase varying with the efficiency of illumination, very dim or very bright illuminations producing greater increases than medium illuminations. Luckiesh, in fact, has used the relative increase in blink rate as a means of determining the optimum lighting conditions for a given activity. Increases in blink rate during prolonged periods of visual activity have also been reported by Carpenter (1948), Tinker (1945, 1949) and Hoffman (1946).

Bitterman, and Bitterman and Soloway have carried out a series of investigations into the change in blink rate with the difficulty of the task, and have failed to find any consistent variations. (Bitterman, 1945, 1946, 1947, 1949; Bitterman and Soloway, 1946 *a*, *b*). In these experiments, the Minnesota Clerical Test has been used as reading material, which, as Luckiesh has pointed out, does introduce speed and accuracy factors not present in his own work. The difficulty of the task has been varied by introducing glare conditions, or auditory distractions. His results have lead Bitterman to claim that the blink rate is too unreliable to be used either as a measure of fatigue or of the difficulty of the task. Tinker (1946) found no significant difference in blink rate in subjects engaged in reading material of unequal difficulty as measured by speed of reading. Brozek, Simonson & Keys found no differential increase in blink rate during one hour, using a task analogous to conveyor-belt inspection, under widely differing illuminations, but did find increased overall average rates under low and high illuminations as compared with medium illuminations.

Hall (1945), investigating the blink rates during conversation and during reading aloud, found a marked reduction when reading aloud. He found, moreover, that blinking, when reading aloud, is not uniformly distributed but occurs at punctuation marks and when turning over pages. Printing the sentences in vertical columns

reduced the blink rate further, and especially at punctuation marks, blinks being now concentrated during the time at which pages were turned.

Lawson (1948), working from figures derived both from his own observations and those reported by Duke-Elder (1942), summarized the extent of individual variation in blink rate on a constant task, and calculated the period of blackout and of impaired vision arising either from lid closure or from saccadic eye movements during blinking. He calculated that the majority of people are completely blacked out for 10 per cent. of their waking time, and that the percentage is considerably increased for the more frequent blinkers. He found, in fact, that 21 per cent. of the population tested had an inter-blink period of less than 1·6 seconds; and 8 per cent. less than 1·2 seconds and 0·5 per cent. less than 0·8 seconds. From this, he argued that people with high blink rates, with a consequent high blackout time, will be less efficient in performing visual tasks and will be more likely to be involved in accidents, for example, than people with low blink rates.

The experiment reported here was designed to test the relationship of accuracy on a visual-motor task to blink rate, and the variations in blink rate during periods of differing difficulty on the same task, in comparison with previous work where the comparison of blink rate with difficulty has been made using different tasks.

II

Apparatus and Method

The investigation was carried out in two parts, the first in the laboratory and the second in a car being driven along a road. The purpose of the field study was largely to check the validity of the laboratory study under road conditions.

A. Laboratory Study.

It was decided that subjects should be set a task which required them to respond to a visual stimulus which placed varying demands on the amount and speed of response required to follow it. If the rate of blinking is related to the amount of visual control of responses, it appeared desirable that the task set should meet the following criteria:—

(i) That the visual stimulus should remain constant at some periods and should change relatively continuously at others.

(ii) That the responses required should be related to the visual stimulus, requiring little or no response to a constant stimulus, and a continuous response to a changing stimulus.

(iii) That the response should be one which would allow the degree to which it followed the stimulus to be scored.

(iv) That the subjects should be unaware that the experiment was concerned with blink-rates. This implied that the task must be such that a plausible alternative explanation could be given, and that the recording of actual blinking should be unobtrusive.

To meet these requirements, a visual tracking task was designed in which the subjects were required to steer, by remote controls, a pencil along a path which at times was straight and at times oscillated from side to side in such a way as to require rapid and fairly vigorous responses to follow it.

The track was provided by drawing a black line, $\frac{1}{8}$ inch wide, on a band of paper 6 inches wide. The line alternated between being drawn straight and in the middle of the paper, and oscillating irregularly across it. A stencil was used in drawing the lines to ensure uniformity for different subjects. Fig. 1 shows part of the first straight piece and of the first oscillating section. The paper band with the line drawn on it was

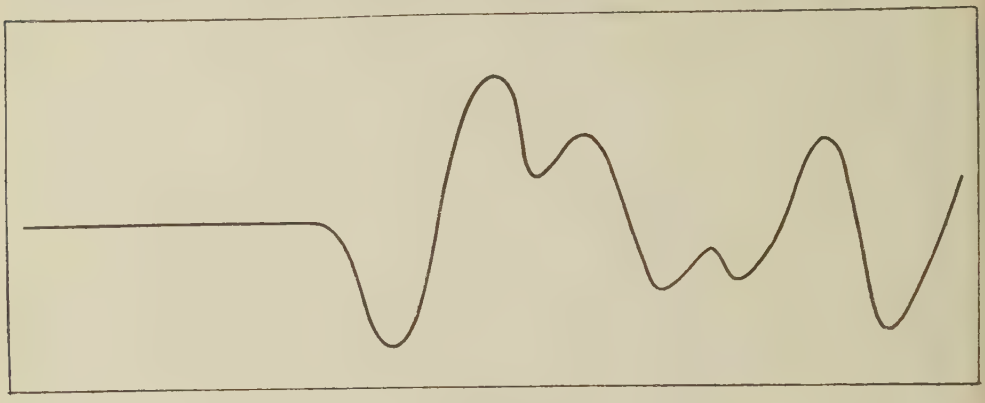

FIGURE 1

Sample of Track

mounted on a standard "long" kymograph. A screen with a window cut in it was mounted between the subject and the kymograph. When the kymograph was rotated the black line moved horizontally past the window from the subject's right to left, at a speed of 100 m.m.s per second. The window in the screen was so arranged that the front of the moving line, to which he was responding, was at the extreme left-hand edge, and the window was of such a size as to allow him vision of any part of the track during 30 seconds before he need respond to it. He was thus able to have anticipatory information on the kind of response he would have to make in the fairly near future.

A pencil, fitted with four different coloured leads, was mounted on a slide, fixed to the kymograph stand, in such a way that it slid easily across the paper band in a vertical direction. Only the writing point and about 2 inches of the pencil were visible to the subject through the window in the screen.

The subject controlled the movements of the pencil by means of an apparatus modified from a Craik Triple Tester. This apparatus consisted of a wooden control wheel, 6 inches in diameter, mounted with its axle horizontal. Rotation of this wheel by the subject rotated an axle on which were mounted two rack-and-pinion mechanisms. One of these was connected directly to a sliding arm mounted immediately behind the control wheel. The pencil was pulled vertically up the face of the kymograph drum by fishing line tied to this sliding arm, and was pulled downwards by return weights. Thus, with this rack and pinion engaged, the subject had direct control of the movements of the pencil, the speed and direction of which corresponded to the speed and direction of the movements of the control wheel.

A small gearbox allowed the experimenter to disengage this direct drive and to operate instead the second rack and pinion which substituted a velocity control for a direct control. Movements of the control wheel when this second mechanism was engaged moved a friction wheel across the surface of a rotating brass plate. This plate was driven by a synchronous motor. Movement of the sliding arm, and thus of the pencil, was now dependent upon the speed and direction of rotation of the friction wheel. If the friction wheel was kept in the centre of the rotating brass plate, it, and thus the pencil, remained stationary. Once off centre, however, the friction wheel rotated, the speed and direction of rotation being dependent on the direction and distance off centre. The subject, by moving his control wheel, imparted a movement to the pencil, of which he could control the direction and velocity. Thus a given movement of the control wheel would start the pencil moving either vertically upwards or downwards at a given velocity. The control wheel had then to be returned to its central position in order to stop the movement. The position of the friction wheel on the rotating plate could only be determined by observing the movement of the pencil. The mechanism was hidden from the subject and the control wheel had no

tendency to centralize itself. Movements of the control wheel were recorded on a modified revolution counter. The gears were so arranged that, in both direct and velocity control positions, rotating the control wheel to the right moved the pencil upwards and to the left, downwards.

The subject's task was, whether given direct or velocity control, so to operate the control wheel that the pencil point remained on the black line on the moving paper. This meant keeping the pencil stationary when following the straight lines, and moving it up and down at different speeds to follow the oscillating line. In effect, the subject appeared to be steering the pencil along a track which was sometimes straight and sometimes very winding. The slopes of the oscillations of the track varied so that at times a slow, gentle movement was required to follow, while at others the pencil had to be moved as rapidly as the apparatus would allow. The pencil point drew the actual track made on the paper, thus allowing the accuracy of tracking to be secured.

The subject was seated comfortably at a table 8 feet away from the track he was to follow. The track was illuminated by an ordinary ceiling mounting of a 60-watt fluorescent tube. This lighting proved adequate, the $\frac{1}{8}$-inch black line on white paper being perfectly clearly visible, without any glare being experienced. The control wheel was mounted on the table, central to his body, and in a parallel plane, and at a convenient height. The subject had an uninterrupted view of the track to be followed over the top of the control wheel.

It has been mentioned above that it appeared desirable that the subjects should be unaware of the fact that their blink rate was being investigated. This precluded any measurement of blink rate by electromyographic techniques. It was accordingly decided to use observers to record the subjects' blinks. An observer was therefore stationed behind the screen and slightly to one side of the window, in such a position that he could see the subject clearly while not himself being obtrusive. All experimental instructions to the subject were given from this position, and the subjects appeared to accept the view that this was a convenient position from which to observe the experiment, from which to change the kymograph tracks and generally to control the experiment. All switches were mounted in this position, out of sight of the subject, and included a key which was depressed each time the subject blinked. Subjects appeared to accept the experimenter in such a position without suspecting his real function there. The experimenter, then, depressed a key each time the subject blinked. This activated an electro-magnetic pen which recorded on the paper on which the stimulus track was drawn behind the screen and out of the subject's sight. This pen was mounted immediately above the moving pencil, so that the approximate time of the blink relative to the particular part of the track being followed could be ascertained. The key and pen were damped with rubber buffers to reduce noise. Residual noise was effectively hidden by the noise of the electric motor.

This method of recording blinks has two main disadvantages. The first of these is that the observer himself blinks and so may miss some of his subject's blinks, and the second is that it is impossible to time the blinks completely accurately, because of the observer's reaction time. A preliminary experiment was carried out to find out whether the error introduced by the observer himself blinking was likely to be significant for the results obtained. To do this, subjects were given a task identical with that of the main experiment. Blinks were recorded by three different observers, additional to the experimenter, all stationed behind the screen. Four subjects were used, the blinks of each being recorded each minute for 30 minutes. It was found that the variation between observers was very small, showing a mean variation of $\pm$ 2 per cent. from the total number recorded by the experimenter. No signicant differences in the distribution of blinks in time were obtained from the records of the four observers. It was concluded, therefore, that though the total number of blinks recorded could not be regarded as completely accurate, no significant bias was introduced into the research by such a recording procedure. During the experiment proper, two observers were used. The first gave the experimental instructions, started the apparatus and recorded blink rate during the experiment. The second recorded, at the end of each minute, the reading of the revolution counter attached, by means of fishing line, to the control wheel. To give a "base-line" of blink rate when no visual task was being carried out, the second observer recorded the blink-rate, using a hand counter, each minute during the period in which the subject was listening to the experimental instructions, but before any practice period started.

The subject, when seated at the control table, was told that the experiment was designed to throw light on the relative efficiency of direct and velocity control mechanisms in tracking, a problem which has been the subject of considerable research. All the

subjects, who were undergraduates, both men and women and all aged between 19–25, and none of whom had acted as subject in an experiment previously, accepted this without question. It was quite clear, both from their behaviour, and from questioning when the experiment was completed, that none of them ever suspected that their blink rate was being investigated, or had any doubts of the authenticity of the reason given them.

When the subject was seated at the table, he was given the experimental instructions, which included the alleged reasons for the experiment, as outlined above, and the statement that he was required to steer the pencil along the moving track by operating the control wheel. It was explained to him that the control wheel would be varied so that on some trials he would have direct control of the movements of the pencil while at others he would be able to control the direction and velocity of its movements. During this period, the second observer recorded minute by minute each blink on a hand recorder. The kymograph was then switched on to demonstrate the kind of track which would have to be followed, and the speed at which it moved. With the track then stationary, the subject was familiarized with the movements of the pencil relative to his movements of the control wheel. Using, first, the direct control, the subject was asked to move the pencil up and down at varying speeds across the face of the track until he reported that he understood the mechanism and felt he could control the pencil movements adequately. The apparatus was then changed to velocity control, and the same procedure followed. When the subject appeared confident that he was familiar, both with the stimulus he was required to follow, and with the movements necessary to steer the pencil, the experiment proper began.

A new, but identical, track was placed on the kymograph and the pencil centralized as the drum was rotated to the starting position, so that the subject started each trial with the pencil on the track. One complete revolution of the drum completed each trial. This took exactly eight minutes. The track, during these eight minutes, was designed so that straight and oscillating portions followed one another as under:

1st minute	straight
2nd minute	oscillating
3rd and 4th minutes	straight
5th and 6th minutes	oscillating
7th minute	straight
8th minute	oscillating

Blinks were recorded continuously throughout each eight-minute trial.

Four trials were given in all at each experimental period, two with direct control, and two with velocity control. The 1st and 4th trials were given using direct control, and the 2nd and 3rd with velocity control. The tracing made by the pencil was too faint to be visible to the subject, though it was quite legible for scoring purposes. It was found that scoring became difficult if more than two tracings were made on one track. A clean track was therefore mounted on the kymograph between the 2nd and 3rd trials. This meant that a pause of a few minutes duration was interposed between each trial; between the 1st and 2nd while the apparatus was being changed from direct to velocity control, between the 2nd and 3rd while the track was changed, and between the 3rd and 4th while changing back to direct control. The amount of movement of the control wheel was recorded on a modified revolution counter, and was noted each minute throughout each trial.

The accuracy of tracking was computed by measuring with a planimeter, the area between the edge of the true track and the pencil tracing. This gave an error score, representing an integration of the amount and length of time of displacement from the true track, with all errors taken as positive.

The number and distribution of blinks, the amount of movement of the control wheel per minute, and the tracking errors were recorded for each subject for:

8 minutes following a straight track using direct control
8 minutes following a straight track using velocity control
8 minutes following an oscillating track using direct control
8 minutes following an oscillating track using a velocity control

and as a "base-line" the number of blinks per minute before any visual-motor task was attempted.

The experiment thus gave varying degrees of complexity in the visual stimulus, and varying degrees of complexity in the required motor responses.

III

Results

(a) *Relative difficulty of the different conditions.*

It is clear that the four conditions of this experiment represent different complexities of task in responding to a visual stimulus. Following a straight line using direct control was extremely simple; nothing, in fact, needed to be done except to steer the pencil on to the track initially and leave it alone. Following a straight line using a velocity control proved more difficult, since this entailed steering the pencil to the track and finding the one position of the friction wheel on the rotating disc at which the pencil would remain stationary. Slight misplacements to one side or the other resulted in a steady drift of the pencil away from the track. Consequently, although the visual stimulus was still simple, a more complex response was needed to perform the task accurately. The oscillating track provided a task when direct control was used, which placed a premium on rapid and accurate responses. The relationship between stimulus and response was regarded by the subjects as simple. The difficulty of this section is due to the fact that the control wheel had to be turned very rapidly, and with frequent changes of rate and direction to keep the pencil on the track. It is what might be called the "pressure" of the response which causes difficulty here.

Steering the pencil along an oscillating track using a velocity control proved to be very difficult. Fewer, and in general, slower responses were required than when using direct control, though quite rapid movements were necessary to follow major changes of direction of the track. The difficulty here lay not so much in the rate of response as in its complexity. The response required to follow the track moving upwards, for example, at 60° to the horizontal using a direct control was a rapid and continuous rotation of the control wheel to the right. The difficulty lay in moving the wheel sufficiently quickly and at a constant rate. To follow the same track using a velocity control demanded that the control wheel should be moved to the right to a particular position. This position could only be determined by watching the movement of the pencil relative to the track and adjusting the control wheel accordingly. As the track frequently changed its direction, very little time was available for such adjustments. There is some evidence that subjects found this so difficult that they were satisfied with only approximate accuracy. If the pencil was moving in the right direction, and at approximately the correct rate, they tended to leave it alone even though it might be running parallel to and not on the track. This was not found when direct control was used. It appears probable, therefore, that the amounts of error made when using velocity control and direct control are not strictly comparable as measures of the difficulty of the task. In reporting the results in more detail, therefore, these two will be kept separate.

The Mean Error of all subjects for each successive minute is shown in Table I. It will be noted that the distribution of errors is broadly similar whether direct or velocity control is used, but with a greater number of errors being made using a velocity control. As would be expected, following an oscillating track gives a greater error score than following a straight one, with both types of control. No measurable errors are made in following a straight track using a direct control.

Table II shows the distribution of Mean Control Wheel Movements for all subjects in each successive minute. The units in which the amount of movement is plotted are the number of feet of movement of the circumference of the control wheel. Since the control wheel could only be manipulated by grasping its circumference, this unit represents the distance through which the subject's hands were moved. Comparison

of Tables I and II reveals a similar pattern of score distribution for errors and movements. Less movement is required, however, to follow an oscillating track using velocity control, though more errors are made.

TABLE I

MEAN ERROR PER MINUTE

SQUARE INCHES DEVIATION FROM TRACK

Minute	*Direct control*		*Velocity control*	
	Straight track	*Oscillating track*	*Straight track*	*Oscillating track*
1	0·00		0·90	
2		2·00		3·20
3	0·00		0·80	
4	0·00		0·62	
5		1·22		2·80
6		0·95		2·32
7	0·00		0·92	
8		1·20		3·00

TABLE II

MEAN CONTROL WHEEL MOVEMENTS

FEET OF MOVEMENT PER MINUTE

Minute	*Direct control*		*Velocity control*	
	Straight track	*Oscillating track*	*Straight track*	*Oscillating track*
1	1·60		10·50	
2		36·54		32·50
3	1·00		7·22	
4	0·52		3·00	
5		34·22		32·00
6		26·00		23·60
7	0·75		5·52	
8		44·00		38·00

(b) *Variations in reflex blink rate.*

The subjects used in this experiment showed a considerable variation in blink rate during the pre-experimental period, though the figures are slightly lower than would be expected from Lawson's figures. The mean blink rate under these conditions was 14·0 per minute, with a range of from 3·0 per minute to 28·0 per minute. The individual readings are given in column 1, Table III. From this it will be seen that considerable individual differences in blink rate exist even in so small a sample of subjects.

The crucial part of this experiment is the variation in blink rate under the different conditions used, which can be interpreted as differing from one another in the "loading" of both stimulus and response. Table III shows the blink rate per minute for each subject under the different conditions. The frequencies given for the four experimental conditions represent the average frequency per minute for each individual for the eight minutes spent under each condition.

TABLE III

BLINK RATE UNDER DIFFERENT CONDITIONS

Subject	"Resting"	Direct control		Velocity control	
		Straight	Oscillating	Straight	Oscillating
1 ..	28	24·0	15·0	19·0	19·3
2 ..	24	19·5	6·6	16·7	9·0
3 ..	23	8·2	1·9	2·7	1·1
4 ..	18	8·5	1·5	6·6	2·0
5 ..	17	12·1	1·1	12·0	1·9
6 ..	11	8·0	2·5	7·0	10·2
7 ..	10	8·2	0·6	6·0	1·9
8	10	10·1	0·5	4·1	1·5
9	6	5·5	0·5	3·0	0·5
10 ..	5	10·9	3·1	11·3	5·9
11 ..	4	7·2	2·1	5·9	4·5
12 ..	3	5·6	1·6	3·1	1·2

It will be observed that there is a considerable and consistent reduction in blink rate for each individual when following an oscillating, or relatively complex, visual stimulus compared with the rate when following a straight, or reasonably simple, stimulus, using a direct control. Using the formula

$$\sigma_{d_M} = \sqrt{\sigma_{M_1}^2 + \sigma_{M_2}^2 - 2\nu_{1.2.}\sigma_{M_1}\sigma_{M_2}}$$

(Guildford, 1942) this difference is significant at the 1 per cent. level, t being 5·39. A similar reduction in blink rate is seen between following a straight and an oscillating track using a velocity control. In this case, however, the difference is a smaller one, and is not as consistent. In one case (subject No. 1) blink rate under these two conditions is the same, while subject No. 6 shows an increase when following an oscillating stimulus. The overall difference in blink rate under these two conditions using a velocity control is statistically significant at the 5 per cent. level, $t = 2{\cdot}42$, in spite of these cases. It will be seen, furthermore, that there is a reduction in blink rate when following a straight line using a velocity control compared with that following the same stimulus using a direct control. On the other hand, the blink rate when following the two tracks is slightly higher when a velocity control is used, as compared with that when using a direct control. The variation in blink rate with the different conditions is shown graphically in Figure 2, where the mean blinks per minute for all subjects is plotted for each condition.

Figure 3A shows the successive minute by minute variation in mean blink rate for all subjects using direct control, and Figure 3B using velocity control. It will be observed that, using both methods of control, there is a regular variation in blink rate with the difficulty of the task, a relatively high mean blink rate occurring when a straight track is being followed and a relatively low one when following an oscillating track. When using a velocity control, the differences are less marked than when using a direct control, but are consistent and in the same direction.

(c) *Relation between blink rate and movement and error scores.*

Comparison of Tables I and II and Figure 3 indicate that the mean blink rate varies in a way which is inversely related to the variations in both error and movement scores. To simplify the comparison, all three variables have been plotted in Figures

4A and B, A showing the relationship when direct control is used and B using velocity control. To reduce the variables to the same range, all individual scores have been standardised by dividing each individual's score per minute by his average score throughout the trial. Scores when direct and velocity controls were used have been kept separate as stated above. Hence, the standard blink score for subject one in

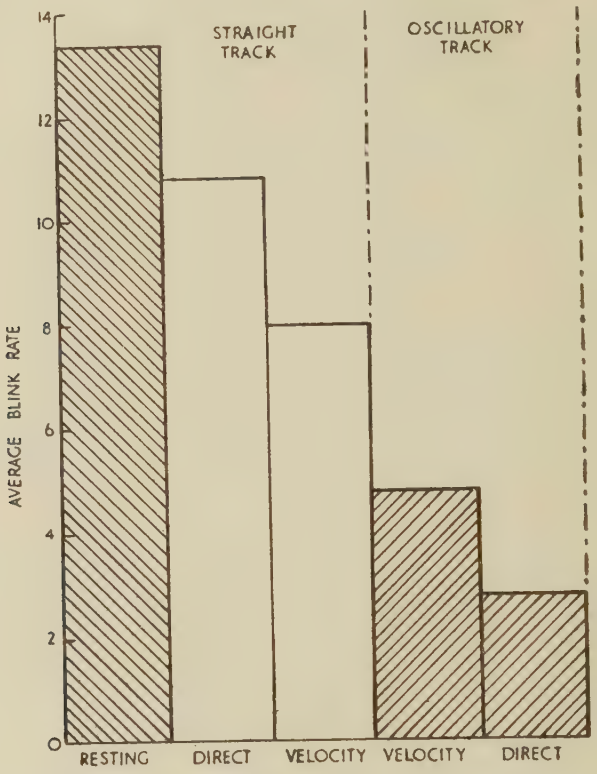

FIGURE 2

AVERAGE BLINKS PER MINUTE UNDER DIFFERENT CONDITIONS

minute one using direct control is obtained by dividing his actual blink rate during that minute by his average blink rate for all minutes using direct control, and similarly for all subsequent minutes. The score therefore represents the ratio of his actual score on any given minute to his average score for a given type of control. A score of 1·00, therefore, indicates an actual score which is identical with the average, a score smaller than 1·00 indicates an actual score smaller than average, and greater than 1·00 an actual score larger than average.

It will be observed that there is a consistent and inverse relationship between blink rate and both movement and error scores. An increase in either error or movement scores is accompanied by a decrease in blink rate and vice versa. This inverse variation is true of all individual subjects, with the exception of subject six when using a velocity control, the inversion of whose blink rate with this type of control has already been noted.

It is not possible from this experiment to define the precise relationship between blink rate and error or movement scores. It is only possible to say that the relationship is an inverse one. The experiment, as designed, gave periods of little or no

response from the subject during which there was a high blink rate, and periods of relatively intense activity, during which there was a very reduced blink rate. The points obtained, therefore, define the two extremes of a curve representing the relationship between these variables. A more graded series of situations, giving graded error and response scores, is needed to enable the intervening points to be plotted.

It is also impossible to decide whether error scores, representing the difficulty of the task, or the movement scores, representing the response load, is the more important variable in determining blink rate. Those situations in this experiment which predisposed to large error scores also demanded large movements. As will be seen from Figure 4, the scores vary in the two conditions in a very similar manner.

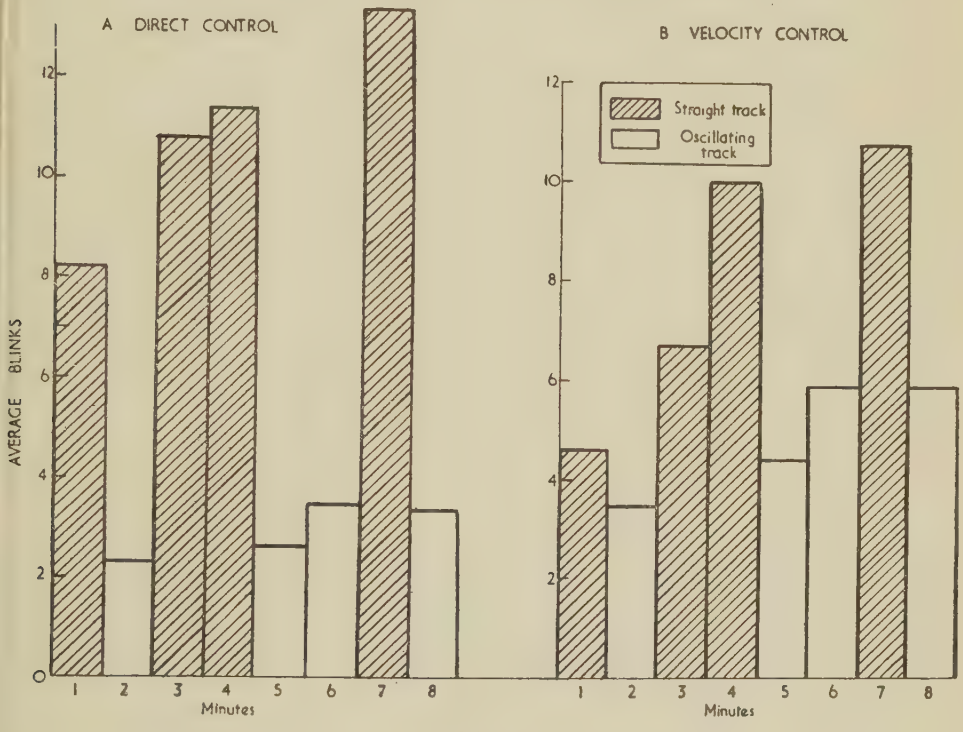

FIGURE 3

DISTRIBUTION OF BLINKS PER MINUTE

It will be observed from Tables I and II that there are signs of a progressive reduction in error and movement scores during successive minutes, under the same type of stimulus. This reduction in errors and movements has its counterpart in an increase during successive minutes in blinking. It seems likely that the reduction in errors and movements represents a learning curve. In any case, the increase in blink rate on the same task, as the task becomes easier, shown by the comparison of these graphs seems additional confirmation of the relationship existing between blink rate and difficulty.

An analysis of the distribution of blinking relative to the part of the track being followed suggests that the timing of blinking is also governed by the nature of the task. Such an analysis is shown graphically in Figure 5. A schematic track has been drawn, representing the straight portions on the one hand and the oscillating portions on the other. The distribution of blinks has been plotted in terms of the total number

of blinks for all subjects occurring at respective portions of the track. Trials when direct and when velocity control were used have been plotted separately. It will be observed that there is a characteristic difference in the type of curve obtained when following a straight track from that when following an oscillating track. This is true with both types of control. When following the straight track with a direct control there is a steady increase in blinking from the beginning to the middle of the period, with a progressive decrease as the oscillating track approaches. With a velocity control, the picture is similar except that the build-up of the blinks is slower. When following an oscillating track, however, blinking tends to be concentrated into two main periods, just before and just after a major change of direction of the track.

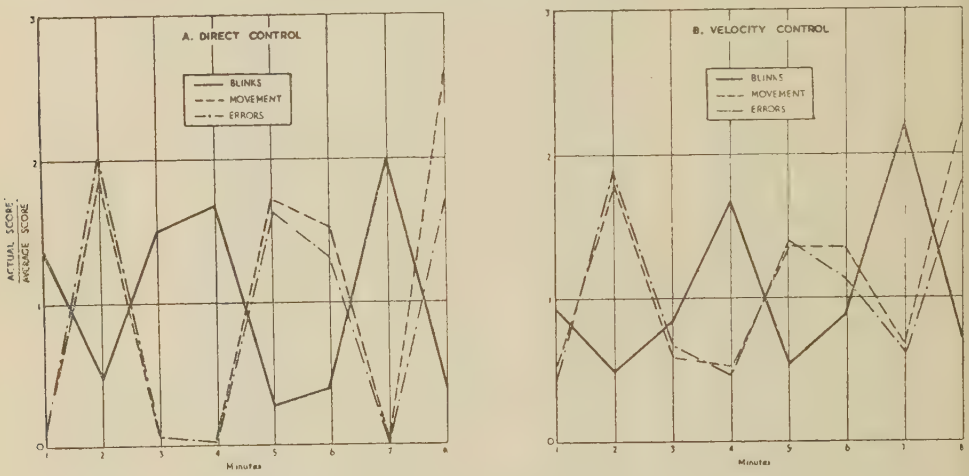

FIGURE 4

COMPARISON OF BLINK RATE WITH ERROR AND MOVEMENT SCORES

These relatively sharp bends in the track are periods of major difficulty and of major activity for the subjects. Throughout the periods of steering the pencil round these bends, of which there were 46 in each trial, there was no blinking at all. When following the more gently sloping parts of the oscillating track blinking took place at a low and fairly steady rate.

Although all subjects show a variation in blink rate with the visual task attempted, individual differences in blink rate are maintained. As will be seen from Table III, subjects with a high "resting" blink rate tend to blink at a higher rate than those with a lower "resting" rate, even during those periods when the overall rate of blinking is much reduced. Product-moment correlations of "resting" blink rate with the blink rate under each of the four experimental conditions range from + 0·68 to + 0·77. These, for this number of subjects, are all significant at the 1 per cent. level.

The results reported above demonstrate an inverse relationship between blink rate and the difficulty of the task as represented by the number of errors made. As the task increases in difficulty, the rate of blinking for each subject decreases. There is no evidence in these results, however, to support Lawson's hypothesis that the relative blink rates of particular individuals would be reflected in the relative accuracy with which they perform a visual-motor task. Correlations of "resting" blink rates with the errors made under each condition range from — 0·014 to + 0·012. When the blink rates and error scores of the various subjects are correlated under a given condition (i.e. following an oscillating track with a velocity control, etc.), the figures obtained

range from + 0·064 to + 0·29. None of these figures approach significance for this number of subjects. The situation appears to be therefore, that, though individuals do differ considerably in blink rate, and the relative order of blink frequency tends to be maintained in spite of marked changes in actual blink rate, associated with the task attempted, there is no relationship between relative blink rate and the accuracy with which such a task as this is performed.

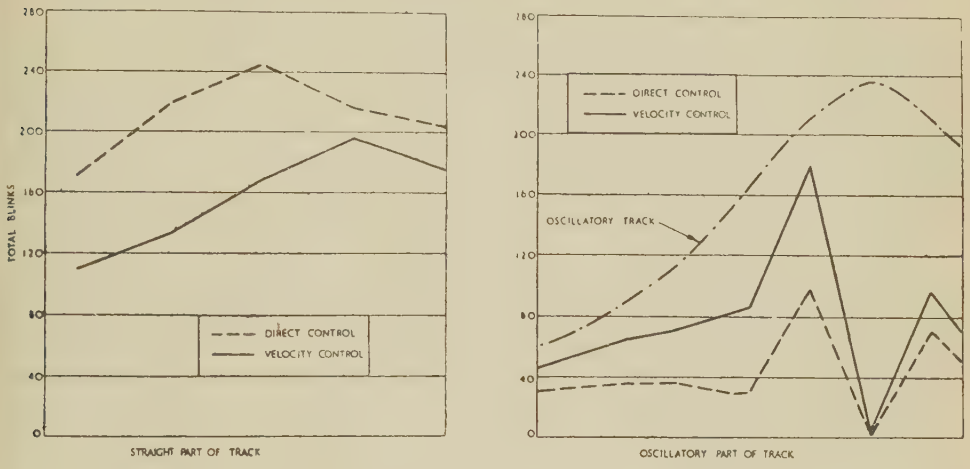

FIGURE 5

Distribution of Blinks at Different Parts of Tracks

IV

B. Car Experiment

As a check on the validity of the laboratory results in a field situation, a cine camera was fitted to a car in such a position that photographs could be taken of the driver's eyes while the car was in motion. Drivers were then instructed to drive along a previously arranged route. This route entailed driving at first in open country, then through a town where very heavy traffic conditions normally exist, and finally into open country once more. Films of the driver's eyes were taken during the three parts of the route.

TABLE IV

Subject	*Average blinks per minute*	
	Driving in heavy traffic	*Driving in open country*
1	0·9	10·2
2	1·3	8·8
3	1·2	7·6
4	0·7	6·8

The results obtained are highly significant even though adequate film records were available for only four subjects. Records for all other subjects had to be discarded since head movements for these subjects were so large as to produce periods when their eyes were not visible on the film. As head movements, and consequent gaps in the records were much greater when driving in heavy traffic, so that gaps were not

randomly distributed, these subjects had to be discarded. Results obtained with the four remaining subjects are tabulated in Table IV.

It will be seen that these results strikingly confirm the results of the laboratory experiment. Blinking is reduced as the stimulus pressure and the difficulty of the task increases.

V

Discussion

The results of this experiment confirm the wide range of individual differences in blink rate noted by previous investigators, and suggest that these relative differences tend to be maintained through changes of conditions which produce major alterations in the actual blink rate. The relative order of frequency of blinking in different individuals tends to be maintained in spite of marked changes in the actual frequency. The evidence strongly suggests, however, that there is no relationship between the frequency of blinking, either during a "resting" phase, or during the actual performance of an activity, and the efficiency with which the individual is able to perform a visual-motor task such as that used.

On the other hand, it appears quite clear that when the activity is such that efficient performance depends upon efficient vision, interference from blinking is reduced to a minimum. This seems to be effected in two ways. The frequency of blinking seems to be adjusted to the demands made by the visual stimulus. Blinking is at a maximum when the visual stimulus does not demand a response, either by itself remaining constant, as in following a straight line with a direct control, or when the changes are such as to have no response significance, as in the "resting" phase of this experiment. The blink rate is then progressively reduced as the stimulus "load," and the difficulty of making an adequate response, increase. It is, unfortunately, impossible, from these results, to decide whether it is the difficulty caused by the complexity of the response, or by the speed at which responses must be made, or both, which control the blink rate, since, in these experiments both varied together. It seems unlikely, however, that the rate of change in the visual stimulus alone is the critical variable, since different blink rates resulted from using direct and velocity control mechanisms with identically changing visual stimuli. This critical variable is clearly to be found in the relationship between the stimulus and the response it demands.

The second mode of adjustment seems to be in the distribution of blinking. In a non-visual situation, such as the "resting" phase of this experiment, and in a visual task making a more or less constant demand, blinks appear to be relatively evenly distributed in time. Ponder & Kennedy and Duke-Elder & Lawson all comment on the relative constancy of the inter-blink period in their observations. When the stimulus-response relationship is a constantly changing one, however, as in the "oscillating track" portion of this experiment, the distribution of blinks changes. Under these conditions, the blinking which does take place, appears to be concentrated around but not during the periods of maximum difficulty, occurring just before and just after these most demanding parts of the task. While the exact timing of blinking is not possible in this experiment owing to the intervening reaction-time of the observer, it appears that there is some anticipatory function in the distribution of blinking. Blinking takes place at those periods when the least foreseeable interference will occur in following the visual stimulus. Hall's observations, that, when reading aloud, blinking is concentrated at punctuation marks, and especially at the

breaks when turning pages, appears to support such a view. Evidence derived from the car experiment, though no more than suggestive because of the few cases involved, suggests that when the stimulus "pressure" is heavy, blinks which do occur, take place during head movements, which presumably reduces the angular velocity of visual stimuli. This is in line with the observations made by Mowrer (1933). It appears possible, from these results, too, that under such conditions the duration of the blink is reduced.

The fact that the blink rate is so sensitive to the task attempted, changing markedly from minute to minute with the task "pressure," may be responsible for some of the discrepancies in previous results. With the commonly used techniques of 5 minute sampling of blink rates at various stages of the experiment, great care would have to be exercised to control the demands made by the task if such sampling periods are to be comparable. There appears, however to be no reason to suppose that the efficiency with which a visual-motor task is performed, is in any way connected with individual blink-rates.

The writer wishes to express his thanks to Mr. B. Waldron and Miss E. Evans of the Department of Psychology, University of Bristol for their help in the laboratory experiment, and to Mr. R. Moore and his staff in the Road Research Laboratory for obtaining the cine records in the car experiment.

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MISCELLANEA

DIFFERENCE LIMINA FOR QUALITY OF PURE TONES: AN EXPLORATORY STUDY

BY

P. DENES and F. A. CARTIER

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It is recognized almost universally that the physical properties of sound waves, their frequency, intensity, duration and overtone structure, have a certain correlation with the perceived characteristics of sounds, pitch, loudness, length, and quality respectively. But these correlations are far from perfect and are intricately interconnected. Few researchers still think of pitch simply as the perception of the frequency of a tone, or of loudness simply as the perception of intensity. Certainly the sensation of pitch is largely determined by the frequency of a sound, but a change of intensity can change the perceived pitch of a tone; the loudness of a tone can be altered without varying its physically measurable intensity. Similarly, we know that quality, once thought of simply as perception of harmonic complexity or overtone structure, is dependent as well on intensity, frequency (Fletcher, 1935) and phase (Chapin, 1934).

It is by their differences of quality that we distinguish between, for example, a clarinet and a trumpet when both are sounding notes of the same pitch and loudness. And it is to their obvious differences in acoustic spectra that we ascribe this quality difference. But it has long been observed that pure tones have easily discernible quality differences, particularly when the difference between their frequencies is great, and, subjectively, these quality differences seem much like those by which we are enabled to distinguish between one musical instrument and another. It is clear then that that aspect of sounds termed "quality" involves more than overtone structure, and is a discernible aspect of sounds with no overtone structure at all.

Some writers have dealt with this difficulty by reserving the term "quality" for the perception of overtone structure, and have said that pure tones have different "timbres" (e.g. Fletcher, *loc. cit.*). Others have attempted to break down even this concept of timbre into various other discernible aspects and claim that timbre is, at least in part, a combined perception of the "volume," "density," and "brightness" characteristics of tones. There seems to be some evidence to justify such a breakdown, but the experimental investigations have given conflicting results (Stevens & Davis, 1938).

It has variously been suggested that the quality or timbre of pure tones may be the result of subjective harmonics, or that the mind may "supply" quality out of memory of everyday sounds. The former suggestion is amplified by the idea that low-pitched sounds seems fuller and richer because many subjective harmonics of low tones would be in the auditory range, while those for high pitches would be too high to be audible.

Hoping to clarify the situation somewhat, the experimenter (F. C.) spent the early weeks of the study simply listening to pure tones, comparing them, and making purely subjective observations which were recorded without regard for logic or theory. Attempts were made to describe and subjectively appraise even the most elusive and vague impressions. Except where noted, all observations were made at equal loudness—always at 40 phons.

It was immediately obvious that it was not only possible to observe quality differences between pure tones, but also to describe these differences in words. The descriptions were always metaphorical and never altogether satisfactory, savouring more of poetry than science, but appeared to be fairly stable; i.e. they seemed equally apt even after considerable practice in listening and after long rest periods over the course of a few months.

Some typical subjective descriptions follow:—

2,000 c.p.s. sounds "sharp and hard" when compared with the "soft, blunt, more comfortable" 125 c.p.s.

500 c.p.s. sounds "purer, clearer, more bell-like" than 125 c.p.s.

250 c.p.s. sounds "somewhat narrower, thinner and not so fuzzy" as 125 c.p.s.

62 c.p.s. sounds "more rumbly" than 125 c.p.s. It seems possible to become aware of the vibrations *per second* of a 62 c.p.s. tone. There is much in common, however, between the qualities of 62 and 125 c.p.s.

1,500 c.p.s. has a more "reedy" sound than 1,000 c.p.s. 1,000 c.p.s. does not give any impression of "reedy-ness."

125 c.p.s. does not seem to have any "bell-like" quality. When compared with 225 c.p.s., the latter has a distinct "ringing, bell-like quality" which differentiates it sharply.

The quality of 31 c.p.s. is rather unusual. But the intensity must be very high if it is to be compared at equal loudness with, e.g. 40 phons of 500 c.p.s. This frequency was therefore not investigated further.

It will be noted that some of the verbal "descriptions" are metaphors which might fall into the classes of "density" and "volume" as explored by Rich, Stevens and others; but other "dimensions" of quality are as readily observable and the range of metaphors which seem applicable to quality sensations appears to be almost unlimited. "Sharpness," "fuzzyness," "sourness," "richness" and other metaphors, once suggested, seem to be just as real aspects of quality as "density" and "volume." It is relevant that some people describe quality differences among sounds with chromatic metaphor, and even ascribe certain colours to certain pitches. The experimenter found it impossible to imagine colour metaphors for quality or pitch, but found it entirely possible, though difficult at first, to imagine taste-oriented metaphors. Once decided upon, the metaphors seemed strikingly apt. It may be supposed that careful experiments might yield difference limina for such "dimensions" and it may well be suspected that such limina as these would be measurements of the validity of the metaphor rather than of the sensation phenomena as normally defined.

When subjectively comparing tones of equal pitch but different frequencies (e.g. 1,000 c.p.s. at 30 d.b. sensation level has the same pitch for the experimenter as 1,010 c.p.s. at 50 d.b. sensation level) there seems to be a definite quality difference (besides the loudness difference). This quality difference does not seem to be like the quality difference between two tones of equal loudness and different frequency.

It is difficult to overestimate the factor of focus of attention on the subjective impressions received when listening to sounds. The experimenter seemed to be able to disregard almost any aspect(s) of a sound and concentrate solely on any other. After much practice at this, however, it became difficult to listen to the totality of a sound. There was instead constant shifting of attention from one aspect to another. It was found to be particularly difficult to listen to the total quality of sounds made up of two pure tones unless they were octaves or were very close together in the spectrum. It appeared that two simultaneous tones could be perceived as separate sounds under certain conditions. Indeed, by concentrating on one of the two tones, the other could be reduced to apparent non-existence.

Out of these and other observations, a problem suggested itself for experimental investigation. The problem was stated thus: Is it possible to determine frequency differential threshholds (difference limina) for the quality of pure tones?

The equipment for the experiment (which was the same as that used during the preceding observations) consisted of two sine-wave generators with maximum harmonic content of 0·25 per cent. or less, and an electronic switching device which, on pressing a button, automatically presented one tone for two seconds, then a silence of about a quarter of a second, and the second tone for two seconds. The sounds were presented through headphones. The maximum harmonic content of the acoustic output of the phones was 0·5 per cent. and was usually much less. There was no audible switching click or surge. Separate attenuators permitted adjustments of intensity so that the loudness of the two tones could be kept constant at 40 phons regardless of frequency differences (Denes & Naunton, 1950). The switching and timing device was a refinement of the device described more fully by the same authors.

The experimenter was the sole subject. All observations were made monaurally with an earphone.

The method was to set the first tone of the comparing device at a base frequency (e.g. 125 c.p.s.) with a loudness of 40 phons, and vary the frequency of the second, keeping its loudness constant at 40 phons. Thus 125 c.p.s. was compared with 135 c.p.s, then with

500 c.p.s., then with 200 c.p.s., and so on. For each comparison, a record was made of the frequency of the tones and whether or not a difference of quality was observed. Choice of frequencies for the second tone were more or less random until the general area of the differential threshold became apparent. Then frequencies were selected with an eye to narrowing the area. It was originally planned to use an equal loudness contour chart and read off the necessary attenuator settings for keeping the loudness constant at 40 phons. Such a chart was made before the experiment began. It was discovered, however, that the experimenter's thresholds and even the shape of the equal loudness contour varied quite noticeably from day to day. It was therefore found to be more convenient and accurate to balance the loudness of a small range of tones to be compared before each session rather than rely on even the most carefully made chart.

The following figures are presented only as an indication of the type of finding looked for in this preliminary work and as some proof of the plausibility of such research. Figures are averages of several trials, but represent both casual as well as careful observations.

Base frequency	*Absolute D.L. in c.p.s. frequency increase*	*Relative D.L. in per cent. frequency increase*
125	Between 40 and 100	Between 31 and 80
500	150 280	30 56
1,000	200 350	20 35
3,000	400 1,000	13 33
4,000	600 1,000	15 25

The second column of the table may be read, e.g. "Comparing with 125 c.p.s., frequencies up to 125 + 40 = 165 c.p.s. showed no difference in quality, but frequencies above 125 + 100 = 225 c.p.s. were observed to be distinctly different in quality. Observations between 165 and 225 c.p.s. were inconsistent."

Summary and Conclusion

A single subject listening monaurally and at constant loudness of 40 phons, compared pure tones of different frequencies for differences in quality. Subjective observations indicated that pure tones of different pitches are also characterized by differences in quality which seem to be much the same sort as those by which we are enabled to distinguish between the sounds of different orchestral instruments.

It was then attempted to determine the minimum frequency-changes which would be accompanied by changes in quality, i.e. to determine the frequency differential thresholds for the quality of pure tones. It was found possible to determine such thresholds, and rough preliminary figures were obtained.

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A CASE OF ABSOLUTE PITCH

BY

A. CARPENTER

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People who claim to be able to place the pitch of any given musical note, without reference to some external standard but purely "by ear," seem to be regarded by many non-musicians as either misguided or dishonest. Yet musicians are quite familiar with the phenomenon, and recognize that a very reliable and accurate "sense" of this nature occurs in a small minority of their number, who are described as having "absolute pitch." This ability is certainly rare, however, and having recently had the opportunity to examine a man who claimed to possess it, and found his claims to be largely justified, I considered the results of my brief examination to be of sufficient interest to publish.

The subject was a research student in zoology and a keen amateur pianist and singer. He comes of a musical family and has been in contact with practical music all his life. The question arose because his research had included frequency measurement, and he had used his unusual ability to name the note he heard, and had then converted the musical labels into frequencies by reference to tables. I was asked, as it were, to "calibrate the apparatus" which he had used; and this I did as follows:—

Twenty-eight forks were collected, covering a range of 65 to 1,300 c.p.s., which, coming from various sources, did not form a coherent series, but were scattered irregularly, yet without major gaps, over the range. The subject, provided with pencil and paper, and his conversion tables, sat at a desk. Behind him the forks were arranged on a table from which the experimenter picked them at random, and sounded them by striking with a rubber mallet. In almost every case one blow was sufficient for the subject immediately to speak, and to write, the name of the note. Each fork was presented twice, separated by a variable number of others. In addition, the note from an electrical oscillator was sounded on two occasions, but in this case the frequency was increased from 602 to 608 c.p.s. between the two presentations. The second note was judged distinctly sharp of the first, but the difference could not be estimated quantitatively, and the two were recorded as the same note. There were thus 58 presentations, which occupied about half an hour. The subject claimed that there was no interference between each note and those which had preceded it, and his claim is supported by the finding that there was neither increase nor decrease in error as the series progressed. The forks and oscillator were calibrated after the subject had been tested, by reference to a crystal-controlled oscillator of undoubted accuracy, using an oscilloscope.

The results in summary are as follows:

(*a*) The mean error of the whole series, expressed in each case as a fraction of the known fork frequency, was 0·025. Since a tempered semitone has a frequency ratio of 1·0595, the mean error was less than half a semitone.

(*b*) Taking into account the sign of each error, the mean was plus 0·015, i.e. an average over-estimate of 1·5 per cent.

(*c*) On the average there was no difference between pairs of presentations of the forks (means: 0·026 and 0·025).

(*d*) The subject did not always agree with himself, however, about the same fork; the average difference between the pairs of estimates being 0·018.

(*e*) The electrical oscillator, adjustable over the very small range of 602–608 c.p.s., was used with the hope of exploring the least nameable difference in pitch, at that frequency. As has been said, this difference was immediately recognized, but could not be quantitatively estimated. The least namable difference appeared to be of the order of a quarter of a semitone. There were two pairs of forks (1) of 255 and 260 c.p.s., and (2) of 998 and 1,000 c.p.s., which were estimated as the

same note on all four occasions in each case (1) 262 c.p.s. (c_1) and (2) 988 (b_2). When the forks of the second pair were presented in immediate succession, however, without the possibility of beats arising, that of higher frequency was immediately picked out as sharp of the other, the difference being 0·2 per cent.

(*f*) This brief experiment cannot claim to have explored the whole audible range, but as far as it went, there was no indication of appreciable change in ability with frequency. The error was approximately constant, the largest error, of 0·096, occurring at 425 c.p.s., and adjacent to some of the smallest.

(*g*) Finally, very rough tests with a beat frequency oscillator indicated that this ability extended in frequency to about 4–6,000 c.p.s. Intervals of increasing size could be recognized up to about 10 kc.p.s.

Of particular interest was the way in which the subject gave his estimates. Whereas the name of each note, e.g. "G sharp," was given at once, the octave came after a very brief interval, and the frequency was then obtained from the tables. Such doubt or hesitation as occurred was never about the name or the place in the octave, but rather about in which octave the note occurred. In fact, no octave errors were made. Bachem (1937; 1948) divides the perception of pitch, in such subjects as the present one, into (*a*) the "chroma"—i.e. the place of a note in the octave—as immediately presented to consciousness as is, for example, the colour "red"—and (*b*) the "tone-height," an approximate but quite independent impression, which gives the octave. The present rather scanty results are in agreement with this hypothesis—incomprehensible though it is to one, such as myself, who lacks the ability of "absolute pitch."

In summary, notes between 60 and 1,200 c.p.s. were immediately named with a mean error of less than half a semitone, the frequencies being given with a mean error of 2·5 per cent. Frequency differences of less than this magnitude could be recognized, but not estimated.

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A SIMPLE STIMULUS GENERATOR

BY

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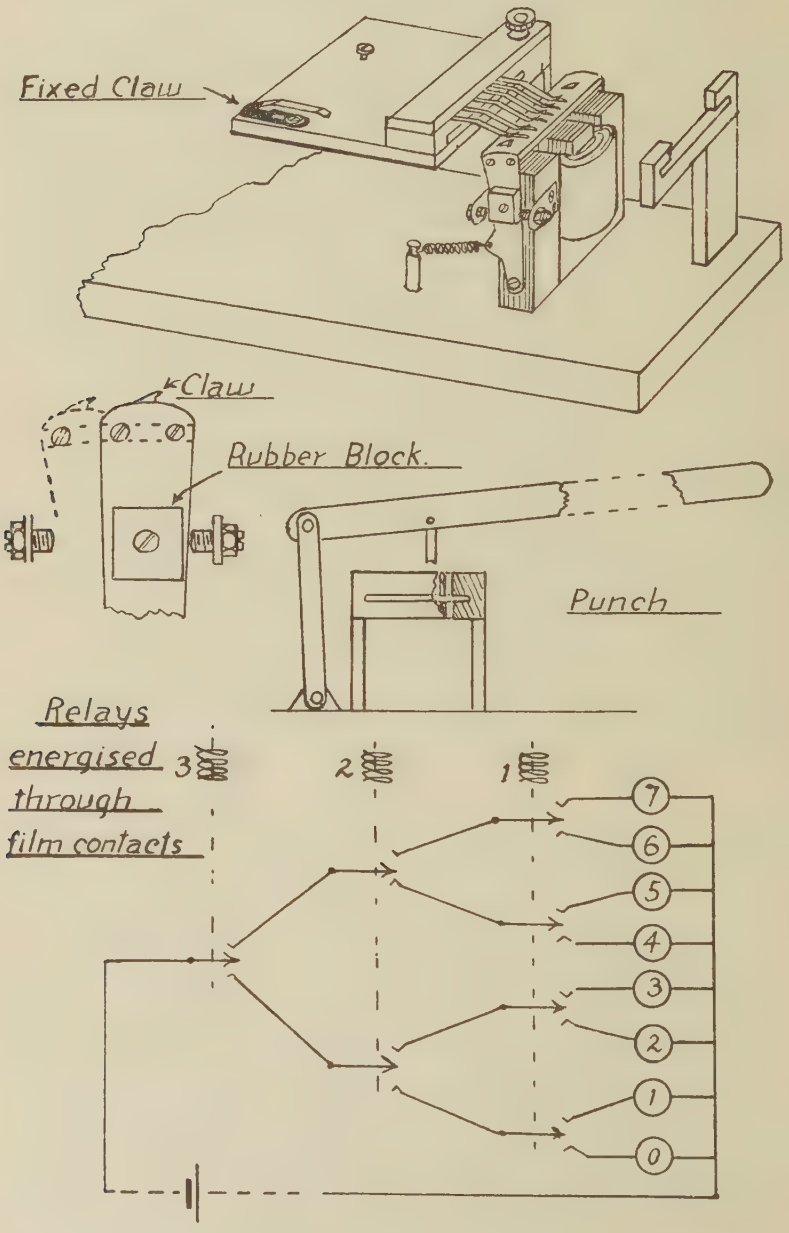
It is often useful to be able to present a prepared sequence of events, such as stimuli, automatically. The device described is easy and cheap to make, simple to use, and has proved very reliable.



The stimuli are represented by holes punched in 35 mm. film or recording paper. Almost any other film or tape with driving perforations could be used, but this is a convenient size, and provides a use for old stock. Claws on the armature of an electro-magnet move the film on one perforation at a time, and contacts are "made" through the punched holes. The diagram indicates the general features, but the following practical points are worth noting.

The electro-magnet is of a type obtainable at present very cheaply from dealers in surplus equipment. But any transformer or choke of similar size could easily be adapted by cutting the stampings and, if necessary, rewinding. A power of about 5 watts is required for reliable action, and the coil may have to be rewound to suit the supply voltage. D.C. is preferable, but A.C. may, of course, be used if the rather loud hum is not objectionable.

The upper contacts are strips of copper foil, 0·004 in. thick, with dimples punched in the contacting ends. The lower contacts, in the present model, are blobs of solder on strips of copper foil fixed with cellulose cement on an insulating layer on the top of the armature. The important point here is that the contact is a sliding one, which not only eliminates the necessity for non-oxidizable contact materials, but also prevents failure due to dirt between the contacts. It can be arranged that contact is made either when the magnet is energized or when it is not.

The claws must be shaped with some care, to ensure smooth engagement and disengagement. Since they slide under the film on the return journey, they should not project upwards more than necessary. A fixed spring claw (only one is required) prevents the film slipping back.

No spool is necessary for the film. The roll is simply slipped on to a rod projecting from an upright member. The sudden drag of the claws is taken up by a spring tensioner, and the apparatus works well with as much as 100 ft. of film. As there are about 65 perforations per foot, no such length is ever likely to be needed. For practical lengths, of the order of 10 ft., the tensioner could be omitted. For such lengths, also, it is far more convenient to let the film run on to the floor than to provide a take-up spool.

The punch for preparing the film happens to be separate from the present model, but could with advantage be incorporated in the device. The punch and die should be hardened and tempered, and the punch should have the usual cylindrical concavity in the end. The diameter of the holes is $\frac{1}{8}$ in., which allows six to be placed across the width of the film.

Finally, concerning the use of the apparatus, six contacts enables six circuits to be made or broken independently, and therefore six different stimuli may be given in any desired sequence, without further complication. On the other hand, by making use of combinations of holes, instead of only one hole at a time, a sequence containing up to 63 alternatives may be constructed (omitting the case of "no hole"). Although this is a larger number than will commonly be needed, it sometimes is convenient to employ the same principle for smaller numbers; e.g. up to three holes will give seven alternatives, and they can be directly recorded in the same code by three pens. But if it is necessary to transform this coded information into a selection of one out of 7 separate circuits—if 7 is the number of alternatives—a decoding mechanism must be added. The principle of a simple type, using relays, is shown in the lower part of the diagram.

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BOOK REVIEW

The Mathematical Theory of Communication. By CLAUDE SHANNON and WARREN WEAVER. University of Illinois Press, 1949. Pp. 117.

Although this is not a psychological work, the current interest among psychologists and other biologists in *Information Theory* justifies some reference to it here. The publishers describe the book as "largely intended for social scientists who are concerned with . . . the broad communications problem, and for students of language," but this is stretching a point, doubtless in the hope of appealing to the wider market uncovered by Wiener's Cybernetics. In fact, however, Dr. Shannon, whose contribution is by far the larger, contents himself with a concise and austere statement of the mathematics of communication, as previously published by him in the *Bell System Technical Journal.* The theory, as he presents it, is therefore conditioned by the engineering problems, to which, as he says, the semantic aspects of communications are irrelevant. Thus, he represents the total system schematically as consisting of an information source, a transmitter, a channel, a noise source, a receiver and a destination. The source generates messages which are coded by the transmitter into signals and despatched along the channel. On their way, they are subject to more or less alteration by "noise"—i.e. by unwanted information characterized only by its statistical structure. Indeed, all the information is regarded statistically. The important entity is not the particular message, but the ensemble of messages. The information content of an ensemble, whether of useful or of "noise" messages, is an entropy, viz:—

$$H = -\Sigma p.\ log.\ p$$

where the p's are the probabilities of the individual messages. Working in terms of entropies is a convenience, whereby, for example, the necessary conditions for the complete reconstruction of the noise-contaminated message can be neatly exhibited.

The engineering bias is obvious from the foregoing remarks. It must be emphasized, however, that Dr. Shannon is dealing with the philosophy of the process; nowhere does he refer to specific "hardware." Indeed, his most concrete examples are concerned with language. For instance, he shows that the redundancy of English permits an almost grammatical sentence to be constructed purely from the statistical nature of the language; and that the redundancy is just small enough for large crossword puzzles to be possible, but not for three-dimensional crossword puzzles. These, of course, are merely illustrative asides. The real meat is in the mathematical concepts and technique, which range from the easy to the difficult, providing something for everybody. Much of the difficult matter could have been made relatively easy by fuller explanation, however.

It is a pity also that no mention is made of the theory of structural and metrical information developed by Gabor and MacKay, or of Fisher's statistical information. All these are logically related, but the general scientific reader still awaits a comprehensive treatise on the fundamental notion of information.

Dr. Weaver's contribution is to explain, in plain English, what Dr. Shannon is talking about, and to suggest broader interpretations; both of which tasks he discharges admirably. In particular, he makes it clear that the exclusion of the semantic aspect is a result of the engineering application rather than an inherent deficiency in the theory. Information without meaning always strikes the newcomer as rather meaningless, if one may put it so; it is therefore good to have this reminder that the separation is not absolute.

In short, this little book is an excellent introduction to the subject, as much for those who merely want an impression of its scope and nature as for those who are interested in studying it seriously.

W. E. H.

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